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Oral History Interviews
Medical Physics Series

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JOHN GOFMAN:
MEDICAL RESEARCH AND RADIATION POLITICS

An Interview Conducted by Sally Smith Hughes

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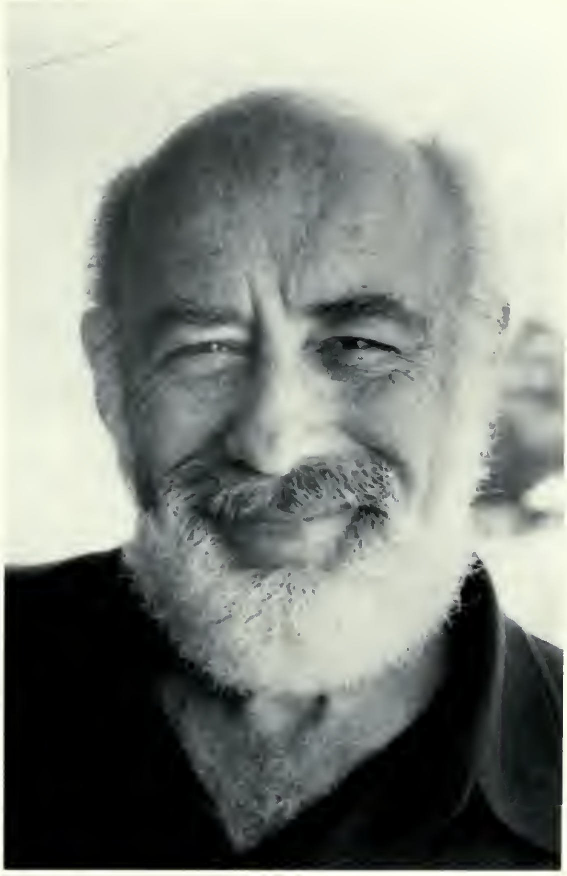
John Gofman at the lipoprotein separation bench,
Donner Laboratory, ca. 1950



John Gofman, 1979

Contents

Acknowledgment iii
Introduction iv
Curriculum Vitae (Gofman) vii

Family Background and Early Education 1
Oberlin College 7
Summer School at the University of Michigan 16
Western Reserve Medical School 17
The Department of Chemistry, U.C. Berkeley 23
Research with Glenn Seaborg 28
Plutonium for the Manhattan Project 42
Ernest Lawrence 51
Medical School at UCSF 53
Job Opportunities 58
Donner Laboratory: Assistant Professor 60
Friction with the Medical School 63
Early Research at Donner Laboratory 72
Research on Coronary Heart Disease and Lipoproteins 73
Research on Trace Elements 91
Atomic Energy Commission Support 95
Hardin Jones 101
The Death of Ernest Lawrence 106
The Donner Laboratory Directorate 109
Lawrence Livermore Laboratory, 1954-1957 115
The Biomedical Division 117
Chromosomes and Cancer 125
Low-Level Radiation 131
Radiation Effects Research 155
The Committee for Nuclear Responsibility 163
Athena Linos et al. 167
Arthur Tamplin 172
Termination at Lawrence Livermore Laboratory 178

Association with the Medical School 182
Early Retirement 183
Institutional Requirements for the Use of Radiation 185
Growth of Nuclear Medicine as a Specialty 189
Use of Radioisotopes and Radiation by the Medical Profession 191
Atomic Bomb Tests 208
Draft Deferment, 1955 214
Donner Laboratory Administration 215
Melvin Calvin 221
The Division of Medical Physics 227
The Institute of Medical Physics 237
Academic Positions at Donner Laboratory 240
Efforts to Establish a Medical Complex at Berkeley 243
Job Offers in the 1950s 244
The Heart Monitoring Device 247
Scientific Method 253

Index 261
Curriculum Vitae (Hughes) 269

Acknowledgment

This interview with Dr. John Gofman is one of several dealing with the development of Crocker and Donner laboratories, within the larger series of oral histories produced by the History of Science and Technology Program of The Bancroft Library.

Besides these interviews, the Program assembles other primary source materials, including the papers and personal memorabilia of scientists and engineers, and the papers of certain organizations with which they were associated. The information in the papers and interviews helps to demonstrate the development of science and technology not only in the western United States, but also in the nation as a whole.

The project was made possible initially by the generosity of William R. Hewlett and David Packard. Mrs. Calvin K. Townsend established the Doreen and Calvin K. Townsend Fund to provide ongoing support of the Program. The University Endowment Fund, National Science Foundation, and National Endowment for the Humanities have assisted diverse aspects of the Program with a series of grants. Further aid has come from the Marco Francis Hellman Fund, established to document science and technology and their relations to business in California. Other donors have included the Woodheath Foundation, the California Alumni Foundation, and the Watkins-Johnson Company.

James D. Hart
Director
The Bancroft Library

Introduction

This transcript is based on interviews with Dr. John Gofman conducted on July 24, August 1, and August 8, 1980, by Sally Smith Hughes. The setting was the living room of Dr. Gofman's home in San Francisco. Topics covered in the interview sessions ranged widely, from Dr. Gofman's education in medicine and chemistry through his wartime work, research on heart disease and radiation effects, and associations with Donner Laboratory and Lawrence Livermore Laboratory. The conversation often turned to his current concern, the human health risks of radiation, a topic on which he has testified at the state and congressional levels and the subject of his two most recent books.

This interview is part of a series recorded during 1979 and 1980 with individuals associated with Donner and Crocker laboratories and, in some cases, the Division of Medical Physics and Biophysics. Donner Laboratory is part of the Biomedical Division of what since 1971 has been called the Lawrence Berkeley Laboratory. Crocker Laboratory no longer exists. The Division of Medical Physics, which in March 1980 became a department, is an academic unit of the University of California.

The medical physics interview series is part of a project funded by the National Endowment for the Humanities to record and document the history of the Donner and Crocker Laboratories, the Division of Medical Physics and Biophysics, and the careers and research of individuals associated with them. In addition to the interviews, documents in the form of correspondence, laboratory notes, reports, and so on were solicited for deposit in the History of Science and Technology Program archives at The Bancroft Library. These materials should be useful in constructing a picture of the growth and development of the fields of medical physics and biophysics, in which the Berkeley research and academic institutions played an early and significant role. They also complement other interviews and documents in the Bancroft collection concerning the overall development of the Lawrence Berkeley Laboratory and its research programs and staff.

Oral history can frequently provide useful information on subjects not easily retrieved from published sources. Hence the questions often dwell on family history, social and economic conditions affecting research, interactions with colleagues, and peculiarities of institutional organization. It should be borne in mind that the interview records what the subject remembers during the interview about what happened at a given place and time. In general, information obtained in response to broader questions is more likely to be accurate than answers on specific events, influences, and accomplishments.

The verbatim transcript of this interview was checked against the tape recordings and edited for punctuation, paragraphing, spelling, verification of proper names, repetition, and continuity. Dr. Gofman reviewed the edited transcript and made minor corrections, additions, and deletions. The index was prepared by Jennifer Snodgrass, who also aided in the final production of the transcript.

Literary rights for this interview are vested in the Director of The Bancroft Library. Any quotation for publication of the material included herein requires the advance written approval of the Director. A request to see the transcript constitutes an agreement to abide by this restriction.

The draft transcript of this interview, as well as biographical data and other background materials, can be found in the papers of the History of Science and Technology Program in The Bancroft Library, and the tape recordings in the Microforms Division. Users wishing to order these materials, or those pertaining to other interviews in the History of Science and Technology series, may do so through the Heller Reading Room.

Sally Smith Hughes
Robin E. Rider
History of Science
and Technology Program
The Bancroft Library
September 1, 1985

Curriculum Vitae
John William Gofman

1918	Born September 21, Cleveland, Ohio Father: David Gofman Mother: Sarah Gofman
1939	B.A., Oberlin College, Oberlin, Ohio
1939-1940	Western Reserve Medical School
1943	Ph.D. (chemistry), University of California, Berkeley
1943-1944	Research Associate in Chemistry, University of California, Berkeley
1946	M.D., University of California, San Francisco
1946-1947	Intern, Department of Medicine, University Hospital, UCSF
1947-1951	Assistant Professor of Medical Physics, UCB
1947-	Clinical Instructor in Medicine, UCSF
1951-1954	Associate Professor of Medical Physics, UCB
1954-1957	Director, Medical Department, Lawrence Livermore Laboratory

1954-1974	Professor of Medical Physics, UCB
1963-1969	Associate Director, Biology and Medicine, Lawrence Livermore Laboratory, University of California
1971-	Chairman, Committee for Nuclear Responsibility
1974-	Emeritus Professor of Medical Physics, Donner Laboratory, Division of Medical Physics, UCB

Family Background and Early Education

Hughes: Dr. Gofman, I'd like to start way back, if you don't mind, with your grandparents. Could you tell me their names on both sides and what they did?

Gofman: I didn't know them. They were European. Both of them, my grandparents on my mother's and father's side, were peasants in Russian Poland. My grandfather on my father's side, his name was Gofman, but I don't know his first name. I was a child when they died. My grandfather on my mother's side, his name was Kaplan. I don't know his first name or the grandmother's; never saw or had any communication with any of them.

My father was brought up in that part of Russia and was from a peasant family and was involved in some of the early revolutionary activities against the Czar. [He] got himself in trouble and was wanted by the Russian government. That was before the successful Bolshevik revolution. He was not a Bolshevik; he was more of a socialist. So he got out of Russia and came to the United States. My mother came to the United States separately. They were among the immigrants who worked in New York City in things like the garment industry at practically zero pay but that got them started in this country and it was an opportunity.

Hughes: That's where they met?

Gofman: They met in New York and married in New York and then decided that the opportunities might be better in Cleveland. They came to Cleveland somewhere between 1910 and 1915.

Hughes: Were immigrants settling in Cleveland at that stage?

Gofman: I presume there must have been some. At least they were brave enough to want to go to Cleveland from New York, so I guess there must have been some word that maybe there were more opportunities if one went to a place like Cleveland. My father had some jobs and finally got together enough money to go into a small business. They had one child, my sister, who is six years older than myself. Her name is Ida. They set up a small creamery business where they sold dairy products. I don't know what the reason was, but for some reason they then ventured off from the creamery business into the furniture business.

I was born September 21, 1918. By that time they were in the furniture business. So I grew up in Cleveland, Ohio. Both my parents worked in their business. I went to elementary school and junior high school in Cleveland. About the time I was in junior high school, the Great Depression came. The large part of the business that my father had was in the black community and the black community was overwhelmingly destitute at the time of the Depression. So my recollection of that period was that, aside from very meager savings, there was just nothing.

Finally my father lost the store because he couldn't keep the mortgage payments up on the store property, and there wasn't any business anyway. So he was unemployed. There was just very, very little income. My sister had just about finished her college work at that time and she got some work as a substitute teacher in the Cleveland schools, so that helped some with income. Things were very, very thin when I went to high school in Cleveland and family resources were just about nil. I didn't really know what to do.

Looking back, you might think I had ideas of an academic career. The only idea that I had when I was in junior high school and high school was whether there was a possibility of earning a living at all. I did think of trying to go to college, got along quite well in school in terms of grades and performance.

Hughes: Any particular field?

Gofman: I just seemed to do best in things like mathematics; that was the field that interested me the most. We didn't have very good science education in our schools in Cleveland at that time. Maybe there was no better anywhere else, but we didn't have good science. Nevertheless the idea of mathematics and science interested me. Since I had practically no guidance--that's nobody's fault, probably my fault that I didn't go seek it--I somehow got the idea that if you wanted to go into science, you became an engineer. Where I got this idea, I don't know, but I did have it at any rate in high school.

So since I had no prospect of thinking of going off to some other city to college because there were just no funds, I found out that Case School of Applied Science annually gave an exam and awarded five scholarships to Case School of Engineering. It's now called Case Western Reserve University.

I did do one other thing, come to think of it. I prepared to take that exam at Case School, but the other thing that I did was to accept some advice from the head of the mathematics department at Glenville High School, in Cleveland. He said that some representatives from Rensselaer Polytechnic Institute, another engineering school, in Troy, New York, had visited the school and said they were anxious to have some people from Cleveland because they hadn't had anybody representing the Cleveland area. They told him that if the school would select someone, it was virtually automatic that there'd be a scholarship and there were also some aid packages such as a job at school. So aside from the idea of taking the Case exam, Mr. Jacobs, the math teacher, said that if I wanted to apply for [Rensselaer], the school would back me and he thought it was a certainty that it would come through.

So I applied to Rensselaer Polytechnic Institute for this scholarship and did nothing else at all about thinking about college, because here was an engineering school. I thought if you wanted to go into science you had to be an engineer. And the school said that I was sure to get the scholarship and some additional funds and I might be able to go since I wasn't going to have any funds from home.

Hughes: What were your parents saying about higher education?

Gofman: Neither of my parents had any higher education. My mother was somewhat indifferent about it. She was very concerned [about whether] I would ever be able to get a job. My father had an enormous respect for higher education, though he had no schooling. That's why my sister got to go to Western Reserve

University [with] the last of the funds that were available to the family. But he had a very high respect for higher education. He also had some amazing abilities. When I was doing math problems and algebra and fields like that in high school, often he would do them in his head because he didn't know the symbolism; I would just state what the problem was in the word problems and he'd do them in his head and I'd do them on paper and we'd come out about the same time.

Hughes: Did you do a fair amount of that?

Gofman: We did a lot of that, yes. It was one close relationship I had with my father. He seemed to enjoy that quite a bit. He had an enormous respect for higher education and encouraged me to try to go to college. He didn't know much about the technical fields or the colleges and we all realized there weren't any funds, but he just said, if you can go, just don't worry about the family, that's fine with us. So he was elated about the prospect of Rensselaer.

But then came April of my senior year in high school and I got a letter from Rensselaer stating that my qualifications were excellent and they would be happy to admit me to the first year class [but] they regretted to inform me that I did not make it on the list of scholarships. So there was an invitation to go to Rensselaer with no scholarship and no job. They offered nothing in the way of aid. Just said there were so many worthwhile applicants, and I believe there were. I had made no preparations thinking of any other school at all, except I knew about the Case exam which came a couple of months later. I had passed the deadlines for applying for all other scholarships because April was when they were announcing. I felt I was a sure thing to go to Rensselaer. So when that came through, I thought, well, I'll try the Case exam and if I don't get that I'll maybe not go to college, try to get some sort of work.

I took the Case exam in June and tied for fifth and there were five scholarships. That awarded me a half scholarship to Case. The tuition, though rather trivial, I think was about $300 or $400 a year. I just didn't have $150 or $200, so I had to turn the Case thing down. That was about the time my high school ended and I decided I'd try to get a job, forget about college for a while, if I'd ever think about it again. My mother told me about somebody she knew whose son ahd gotten a job. He'd looked a long time and he'd gotten a job and was earning about $15 a week, and wasn't that wonderful.

So I went out to try to get a job. I went to all the companies in Cleveland. I was valedictorian of my high school class and had excellent grades in the science courses we did have, chemistry and physics and math. I went to things like the chemical companies in Cleveland and other mechanical industries. I did this for about six weeks. There was no work at all; in fact, they sort of laughed at you. They said, "We can get a college graduate or even a Ph.D. to do the work, so why would we be hiring high school graduates?" Well, I tried everything--getting a job in a store, as a janitor or whatever--but there was no work, just no work.

So finally I knew of some people who had been selling things store to store or door to door and a friend of mine, Howard Metzenbaum--you may know his name; he's now Senator from the State of Ohio--Howard and I talked about it. He had an old broken-down car, so we bought up some goods that we could sell, like aspirin tablets on cards in packages that people could rip off, combs on cards, razor blades for sale in stores, and we were able to buy Eberhard-Faber pencils at a very good [price] and we could sell them to offices--an automobile store would have an office and they'd need pencils.

So we went out on the road traveling. We slept in parks generally, in his old car. We were able to make, after expenses, about $15 a week. That was a lot of money, but it wasn't a very great life--although we got to see a lot of the Midwestern states.

So Howard and I did that. Then he had decided to go to Ohio State University, which was free. He was going to be a lawyer. As I say, he's now a U.S. Senator, became a very successful attorney and businessman. We worked for about the remaining six weeks of that summer, because the first five or six weeks I'd tried to get a job. We did sell some things and we had a good enough margin and our expenses were virtually zero except for gasoline, because we didn't sleep in hotels, we slept outdoors most of the time.

Oberlin College

During the last part of that summer, my sister said she'd talked to someone and asked if I had thought of going to Oberlin College, that they had a very liberal policy about working your way through. And I said, "But that's a music school, isn't it?" She said no, it's got a conservatory of music but it's also got a liberal arts college. And I said,"But that's not an engineering college. I want to do science, I don't want to go to an arts college. So that's how much I knew about what liberal arts meant; thought it was an art school. Just had no guidance--as I say, my own fault.

So I said, well, I could go down and find out. I was thinking then if Howard went back to school in the fall, maybe I would just keep going out on the road myself selling things, because it was the only work I could get. At any rate, I went down one day to Oberlin College and went to see the director of admissions, a fellow by the name of William Seaman, and he said, "You're a little late applying for a scholarship. School's about to start in about three or four weeks. You're a little late applying for admission, but we can take in a certain number of late admissions. We have one program that might interest you: We have a thing called the student aid fund, and any student is eligible to get up to half tuition from it. Because your record is good we can do this for you. We can give you your half of [the year's tuition from] the student aid fund from the first semester, as a special thing; then if you can then manage the remainder, the other half of your tuition, in the second semester, at least you could see about it. We could also maybe get you placed where you could get a job for your room. We can't get you a job for your board because they're too rare. There's just so much competition."

By then I'd quickly found out a lot more: You could go to a college rather than an engineering school and still study science. So I just elected I would take this offer from Oberlin. They did get me a room where I was able to earn my room by doing furnace stoking and garden work. Then the other problem was how to eat. The books weren't all that expensive and I had by then about $150 saved up from the summer work. I had the room job and there was a way to get your board, except for occasional meals, by being a substitute. A lot of guys who had waiters' jobs or dishwashing jobs wanted to study some evening or go to a dance or something like that, so you'd do their job for them. I got to eat about three-quarters of the time and the rest of the time I ate in my room. [I] managed to get through the first semester and then had to use $125 of my funds to pay the second semester tuition.

I did fine at Oberlin. I guess it was the second semester I even got an additional job, with a professor of history, from the National Youth Administration funds--part of Roosevelt's recovery program--I was enrolled in 1935. Interestingly enough for your project, [the job was] with Professor Arthur Fletcher, from whom I was taking a course in American history. He was writing a history of Oberlin. In the administration building they had in the archives just packages of all the correspondence and records of Oberlin back to its founding. My job was to start sorting through all these papers and files and letters and making some order out of [them] for Professor Fletcher, which I did for the second semester. It was a godsend because it paid $15 a month additional and that was just great.

Hughes: How were you doing academically?

Gofman: I did fine at Oberlin academically. I was first in my class that year. That meant that because I was doing that well academically--it didn't mean I could get a full scholarship, but it did mean I could get the full amount of student aid, which was half tuition for the next year.

Hughes: Was your thinking taking any particular direction in terms of--?

Gofman: I took chemistry that first year and a lot of math and I was thinking I would like to go into science.

Hughes: But you hadn't defined it any closer than that?

Gofman: Not any closer; just science was the main idea. It was just the sort of thing I liked to do. I did fine in chemistry and liked it. Of course the only thing I kept wondering about is, is there any work for people who get into science?

At any rate, I finished up the first year at Oberlin. Howard was going back out on the road that summer and I went out with another friend of mine. By then we were more adept at it and we managed to save about $200 or $300 from the work of that summer after the first year. So I was assured I could go back to Oberlin. But by then I was beginning to think, well, maybe I could go to some better school.

I met a fellow in Cleveland who was home from Harvard. He had some concession to deliver the New York Times at Harvard University. He said he could get me some work in that [line] if I could get admitted to Harvard, which seemed like it might be a better place to go than Oberlin. So I applied to Harvard and I got admitted to the second year. I'm trying to recall the details. They said if my record was good enough, my tuition would be refunded for that year, but they wouldn't commit to a scholarship. But this job that this fellow said he could get me helping deliver the New York Times would have paid several hundred dollars.

But by then my father, who was quite young, had had a coronary. He was in his early forties. He recovered from the first coronary. That summer I was out on the road most of the time. When I came back in, he really didn't feel well and he said he just didn't think he was going to make it. That is a thing I learned later in medicine is a real phenomenon. It's called angor animi; it's a sense of impending death. It's relatively common in coronary heart disease, all of which I learned many years later. But he said, "I just don't think I'll live long."

Hughes: Why particularly coronary heart disease?

Gofman: I don't know. I really don't know the mechanism of it medically, but it's a sensation of anxiety of a special sort. It's not a fear of death--he was very calm about it--it's an anticipation of death. It may well exist in other diseases, but I've run into it in my reading and studying of the problem of coronary heart disease. Whether or not that in any way influenced me to later study coronary heart disease, I don't know. I rather doubt it and I'll tell you why.

But at any rate, I thought about it. I thought, if he's really that sick, should I really be going off to Boston? I decided I would ask Oberlin if I could be readmitted. I had already told them I was going to Harvard. Oberlin said yes and I told Harvard I wouldn't come.

So I went back to Oberlin and I was able to get my room job again. I had my half tuition and I had a couple hundred dollars or $250 saved up from the summer. It sounds sort of silly to say $250 saved up, but that to me looked like a bankroll. I mean, it was a lot of money when you consider that was enough to get you through a year of school. I was very lucky; I got an NYA job again, but this time [with] Professor Luke Steiner, who later became the head of the chemistry department. He has been a very, very great influence in my life. Sadly enough, he died last year at 79.

Steiner was a physical chemist and he had some research going on in the quantitative aspects of the absorption of various organic compounds in the vapor phase on solids such as silica gel. He had a very neat apparatus. He was a good glass blower and I got a job with him to help make measurements on his equipment. It was a great thing because there I was getting personal tutoring in an aspect of chemistry, actually taking research data that he meant to use for his research, so it meant care.

So I worked about 30, 40 hours a month on that project. You only got paid for 30, but I worked more and I had my room job again. I also lucked out and got a job doing pots and pans for my board. I had my food and my room and I had enough money left over for tuition. I still had that $15 a month from the NYA job, so I was really able to make it quite easily through that year, only because I didn't have to help with my family.

In the first month of that year, my father did die from a recurrent coronary. I completed that year at Oberlin, and again went out on the road between the sophomore and junior year. By then I had decided I would go into chemistry.

Hughes: Was that largely because of your contact with Steiner?

Gofman: Yes, I think it was, very definitely. I was sort of hovering between going into mathematics [or] chemistry, because I liked the math work very well. People said there was no work for mathematicians, that you had to be a professor, and professors didn't die that frequently so there wouldn't be any jobs.

Hughes: What were you thinking of as work in chemistry at that stage?

Gofman: At that stage, my idea of work in chemistry was to get a job in some chemical factory. That was what I thought was chemistry. If you were lucky, you could get a job in some chemical factory. In fact, there were brochures put up in the chemistry department from the Institute for Paper Chemistry. It was somewhere in Wisconsin. One could, after getting an A.B. degree, go there and get a master's in paper chemistry. And that seemed like an interesting possibility, because there was a special area of chemistry.

Hughes: Were you talking to Steiner about these kinds of things? Was he guiding, giving you advice about your personal life?

Gofman: Yes--well, to some extent. I was very, very shy about ever asking. I'm very shy in general about asking people and don't tend to talk to people too much about what I should do. I've always felt somehow they were too busy. I rarely would ask him things beyond what I needed to know in the research, although he was very generous with his time. I did talk to him about the Institute for Paper Chemistry, and jobs. So I worked with Steiner my second year. I worked all three years--second, third, and fourth years--in his lab.

Hughes: By this time were you doing extra reading in chemistry?

Gofman: Some. [Steiner said,] you ought to be reading the literature some and finding out what we know and don't know about the mechanism and see if our data fit any model of what might be going on in these gels. So I was beginning to read and make some calculations and try to understand the actual chemistry. I hadn't yet had physical chemistry formally; that came in the senior year. Well, that was Oberlin. I majored in chemistry.

Hughes: Did you make any friends that had any particular influence on you?

Gofman: I roomed with two fellows successfully, both of whom were physicists. One was Harry Polster, who became a physicist, and the other was Robert Meijer, who was a professor at an eastern college, was a physicist there. They were both trying to tell me to go into physics, but somehow I didn't think physics was very good at Oberlin. The reason was I had a poor introductory physics course at

Oberlin, so it turned me off, although I did decide the senior year I would take a very advanced course in electricity and magnetism because the professor was very good, and I did. It was a five-unit course. But these fellows I lived with were very influential.

My third and fourth years, I had even better jobs for my board. The third year I worked in a small tearoom where I waited tables and had excellent food and earned about $5 a week in tips. So I was rising on the economic scale. Senior year I had an even better job running an electric dishwasher, which only took about 20 minutes, in a women's dorm.

I kept my job with Dr. Steiner. I decided I would be a chemist, try to get a job in industry somewhere. About the junior year, Dr. Steiner started talking to me about going to graduate school, which I knew nothing about. I thought you'd finish your course and you'd get specialty training like at the Paper Institute. He said you could work for a Ph.D. degree. I really didn't know what all that meant. Well, he kept talking about it. He had a Ph.D. degree in chemistry from Yale University. He said then I might even think of trying to get a job in some college. That was all just so remote, wild, distant from anything I understood, that I said, well, I might think about that.

About the end of the junior year, one of my friends, a fellow I went out with on the road in the summer, by the name of Greenfield, said he was going to go to medical school and had I ever thought of medical school. He was going to Ohio State University. He was in the third year, too. I said no, I hadn't thought of going to medical school. Besides which, it's very expensive, isn't it? And he said, well, yes, it's expensive, but you know you might be able to make it. So I thought about this idea. The idea of working on medical problems from a chemical point of view seemed attractive. So I took six weeks off the summer of the junior year, since things weren't so pressing any more financially. Because I had these various jobs, I didn't have to stay out on the road all summer. By then we were even better at it on the road, making about $30 a week net.

Summer School at the University of Michigan

I took six weeks off and went to University of Michigan at Ann Arbor. I don't know what motivated me then, but I was really motivated in that six weeks. I went to take a course in biology. I couldn't fit it in my Oberlin schedule. That had an afternoon lab but it had just one morning lecture. Also [I] hadn't had a chance yet to take differential equations, so I took differential equations for credit and the biology for credit. Seven in the morning there was a course in biochemistry at the medical school. So I sat in on that, because I thought I'd like to see what this stuff is all about that you might get into in that form of chemistry.

Then there were two other courses that interested me. Kasimir Fajans is an elegant physical chemist. He was giving a course on crystallography and I was intrigued with it, so I sat in on Fajans' course on crystallography. I had just finished organic chemistry and there was this very famous organic chemist at Michigan, whose name was Werner Bachmann. He was giving a course on organic synthesis theory, and so I sat in on his course. So I was in courses all day long from seven o'clock through to my lab in the afternoon. Then I had to go home and do my studying. I think I worked harder that summer, but I enjoyed it and I went to everything; I didn't miss anything. I even studied the courses I wasn't taking for credit. But the advanced organic and the biology and the biochemistry did intrigue me toward something in biological sciences, which I had not considered at all till then.

I came back to Oberlin the senior year. Steiner was still talking about applying to graduate school. He said Arthur Campbell--I've forgotten the name of the second one--two graduates from Oberlin had been given teaching assistantships at the University of California and they were both doing well. He said, "I think if you apply, we could get you a teaching assistantship at the University of California because Oberlin has now established sort of a good reputation with the last two doing well. You could go to graduate school at the University of California, especially since you seem to be more interested in physical chemistry than organic chemistry." His project was a physical chemistry project. I did lean that way. So I thought about that and about applying.

Western Reserve Medical School

This idea of medical school recurred. At Christmas vacation--I really can't explain it to you, Sally--I just got this idea in my head that maybe I ought to find out about medical school. So I went over to Western Reserve Medical School. Now

I know this doesn't sound credible but it's just the way it was.

I saw a woman by the name of Miss Brown, who was the dean's assistant. She said, "What would you like?" I came in and I said, "I'd like to apply for medical school." She looked at me and said, "Apply? It's already beyond the time of applications. When did you decide you wanted to go to medical school?" So I said, "A couple days ago."

She said, "Where are you from?" I said, "Oh, I live here in Cleveland." She said, "Where do you go to college?" I said, "I go to Oberlin." And she said, "How are you getting along at Oberlin?" And I said, "Pretty well." And she said, "Do you have all the prerequisites for medical school?" And I said, "Well, I don't know what they are, but I don't think so. I do have a lot of chemistry." And she said, "Well, chemistry's fine. Have you taken biology?" I said, "I took one course in biology in summer school." "Have you taken zoology and vertebrate anatomy and embryology?" I said, "Oh, no. I haven't taken those. But there is still a semester left at Oberlin. I could take some of those then." So she asked me my grades and since I had very high grades--I was still first in my class at Oberlin on a grade-point average--she said, "Wait a minute." She came back and said, "Dean Sollman will see you."

Dean Torald Sollman was then about 75, but he was still dean of the medical school at Reserve. He was a world-famous pharmacologist. She let me go in to see him. He peered over his desk and said, "I hear you just decided you'd like to enroll in medical school." I said yes. "What were you thinking of doing before?" I said, "Oh, I thought

I would be a chemist." That was just a lucky break because, as a pharmacologist, he was interested in chemistry. So he said, "Have you taken the medical aptitude test?" I said, "What's that?" He said, "Well, it's a test we give to see if people are suited to..." I said I didn't even know it existed. So he said, "Well, we have some copies of it here. I'll give you one of them and you just go over there in the next room and sit down and take it." So I did.

Before I left, Miss Brown asked me to fill out the application. She said,. "Not very good prospects, because virtually all the places are taken." So I filled it out. Just as I say, a will-o'-the wisp idea to walk over to Reserve and apply for medical school.

I think we had the semester system and just toward February was when the new semester started. Toward the end of January I got a letter from Western Reserve stating that I would be admitted provided I could make up my deficiencies. Well, it turned out I couldn't make up my deficiencies. I hadn't had vertebrate anatomy and I hadn't had embryology and I had only that short biology course. So I went back to see them at Reserve and said that I could take embryology and in the summertime I would dissect a cat by myself--that was what vertebrate anatomy was, cat dissection. So they said that would be all right. So I came back to Oberlin and decided I'd go to medical school.

The next thing was how in the hell was I going to finance this, because tuition was about $400 at Reserve. But somebody put me in touch with a foundation called the Cleveland Foundation, which gave out scholarship grants and loans. I went to see them and they said they would lend me $500 for my first year of medical school. (Interestingly enough, when my work on coronary disease became known, they cancelled the loans that I still had outstanding. I paid them back anyway; I wanted to make sure that someone would get to use the money.) They were very nice. They loaned me this money for the first year at Reserve.

So the second semester at Oberlin, I went to see Dr. Robert McEwen, who had written a book on vertebrate embryology, and I said I would like to take the embryology course. He said, "Do you have all the prerequisites?" It was a sign-up course because it had a lab and everything. I said no. And then I told him I had just that one summer course in introductory biology, not even as much as first-year zoology at Oberlin. He said, "I just don't see how you'll understand what's going on." I said, "Well, are all the spaces filled?" He said, "No, it isn't that the course is full. We could put you in the lab." And I said, "Well, gee, I would appreciate it; if I wouldn't be in anybody's way, I could take the course. I'll just take my chances." So he agreed and I did fine. I got the highest grade in that class.

We got to be very good friends because it was a tremendously exciting course, embryology. I loved it. In fact, we were talking then [about whether] there was something chemical that resulted in the organization from just a group of cells into a region that's the nervous system, and a region that's limbs. There had already been some experimentation on transplanting pieces from the frog embryo from one region to another and demonstrating you could induce the formation of a second nervous system in a frog.

Dr. McEwen and I talked about it and [decided] it's got to be something chemical. There had been an idea that there might be a chemical called the organizer. Nobody knew what it was. So that summer when I stayed at Oberlin to dissect my cat, which I had to do for the vertebrate anatomy, I did a little research on frogs' eggs, trying to isolate some chemicals. And I did isolate some fractions and tried some experiments but never succeeded in proving much. But it was an intriguing thing.

Also that summer, since I was there, Dr. [Harry] Holmes, the head of the chemistry department, not Steiner, had a job open. He and Ruth Corbett had been the original isolators of vitamin A. He was on a project to look in bone marrow for the compound that might be involved in stimulating white cell production. So he wanted to know if I wanted a job to try to fractionate some bone marrow and look for this compound. So I had my cat and my frog work and this job with Holmes, which was intriguing. And I did isolate a pure crystalline compound that was later identified by someone else as a compound called batyl alcohol. Really quite intriguing to me to actually have something happen in a lab, find something, although I'd made a lot of measurements on Dr. Steiner's project before.

So after that summer I went off to Western Reserve Medical School and I did fine. But what I sensed, by the end of the first semester, was that this was going to be a highly practical, practice-oriented education. By then I had said, if I'm going to be in medicine, I don't want to be a practicing doctor, I really like to do research.

The professor of anatomy at Western Reserve, a man by the name of Norman Hoerr, had just become professor of anatomy at Western Reserve because Dr. T. Wingate Todd, an old Scottish anatomist, had died the year before. So Hoerr came from an exciting school in Chicago known as the school of Bensley and Gersh, who were doing the beginning work on freezing tissues and doing cytochemical studies of tissues, enzyme studies in tissues, in slices--just the beginnings of that. So here was a guy in anatomy who had been doing microscopic chemical anatomy. This was a very exciting person and a great teacher of anatomy.

I went to see Hoerr during the midyear break and said I really thought I ought to try to get some more chemistry. He had gotten a Ph.D. in this cytochemistry work. I said I thought I ought to combine it with my medical education. He said he thought it was a great idea. And I said what worried me was, if I went off to study chemistry for a while, would I ever be able to get back into medical school?--because I did want to finish it. He said, "Listen, as long as I'm in this school, you'll get back into Reserve."

So then I went down to see Steiner and said did he think there was still an opportunity at the University of California, since they had these two [Oberlin graduates]--Jim Goodrich, that was the other one, besides Arthur Campbell. He said, "I think there's a good chance they would take you. We'll be happy to back you." So I applied for a teaching assistantship at UC Berkeley.

Hughes: Not really knowing anything about Berkeley...

Gofman: Nothing. Steiner said it was a good school for physical chemistry. That is all I knew about Berkeley. It was off in that place called

California. I had seen the Mississippi but that was my limits.

The Department of Chemistry, U.C. Berkeley

Hughes: This is presumably about 19--?

Gofman: 1939, 1940. So I applied to the University of California department of chemistry for a teaching assistantship and I got it. So I asked for a leave of absence from medical school. I placed first in my class at Western Reserve Medical School in grades. Dean Sollman didn't know if it was such a good idea. He said I could get all the chemistry I wanted at Reserve. But he finally said okay and he gave me this leave of absence and I came out to Berkeley.

Oh, we got married that summer--Helen Fahl, who I'd gone with at Oberlin the last two years. We got married that summer; came out to California in the fall of '40 with one year of medical school background.

Miss [Mabel] Kittredge was the lady who ran the department of chemistry. Miss Kittredge was her maiden name; she was married but used her maiden name. G. N. Lewis was still dean of the College of Chemistry and so I went in to see Miss Kittredge when I arrived.

Hughes: Did the name G. N. Lewis mean anything to you?

Gofman: Oh, yes, everybody knew the name of G. N. Lewis. I did too because he was textbook stuff. I wasn't terrified at the beginning at Berkeley--that first week, that is. You know then we used to come to school in August at Berkeley; I think it had something to do with the agriculture or harvesting that school started early and finished in early May.

I got there in August and introduced myself to Miss Kittredge and she said, "Well, Professor Lewis will see you." He saw me that day and said, "Glad you're here. You may take a few courses, but we expect people to start out on their research right away." That shocked me because I'd had so little chemistry. I looked at the catalogue and it had all these courses that I'd never heard of and how was I going to start out on research without taking all these courses?

Then I got to talking with some of the other people in the chemistry department, graduate students such as Art Campbell, the guy from Oberlin, and Jim Goodrich, who had been there. You take a few courses that everybody takes, but you're expected to take your prelims by the end of the first semeter, certainly some time during the second semester, and to be well started on your research by then. I was then beginning to really get a little terrified, expecially since one of the guys in my graduate class was Leo Brewer. Do you know him? Professor Leo Brewer in the department of chemistry?

Hughes: I've heard the name, but I don't know him.

Gofman: Very elegant thermodynamicist. He and Ken Pitzer revised Lewis and Randall's Thermodynamics, the classic book. Well, Leo was one of my co-graduate students. So we were meeting the other graduate students and here was this Leo Brewer who had come from Cal Tech and he'd had everything in chemistry and he knew everything. He said, "No, I'm not going to take any courses. I'll take my prelims in the first couple weeks." The rest of us were just horrified [and thought], well, maybe we shouldn't have come to this place. From Oberlin my chemical background was so little.

So I enrolled in a couple of courses. [I] also decided I'd better learn some physics, so I enrolled in a course that [J. Robert] Oppenheimer was giving. There were three of us enrolled in that: a fellow by the name of Spofford English, who subsequently became a professor and then went off to the Atomic Energy Commission, and Robert Duffield, who was a professor at Illinois later, then became director of the Argonne Lab, and I....

Hughes: I was just wondering why you chose Oppenheimer's course of all the ones that could have been chosen.

Gofman: Well, I think everybody sort of suspected you should know all the other stuff, the undergraduate stuff, so we took this graduate course. Also because he did deal with nuclear matters and all three of us had elected a research project on nuclear chemistry.

Hughes: How did you do? You hadn't had any leanings particularly...

Gofman: Nothing. I'll tell you how I did it. I found out what was expected of you. G. N. Lewis had seen me for five minutes and said, "Get started on some research." Then [I] found out you're supposed to go see everybody in the department who is looking to take a graduate student and see if they have a problem and if you think the problem's interesting and if they want you to work with them. So I went to see William F. Giauque, who had his low-temperature thermodynamics work, and Pitzer and Rollefson and Olson and this guy Glenn Seaborg, who was just working on artificial radioactivity.

It struck me when I talked with him that gee, this might be just right for me to get into, not because I was primarily interested in radioactivity or nuclear physics, but the whole idea of tracers and better tracers and what it might mean in biology and medicine if I were going to go back to medical school. So I said, "I'll work on nuclear chemistry because I think it's the thing that might be a good bridge for the two fields." This was 1940 and in '39 fission was discovered.

Somebody had thought they had a radioactivity made from thorium, by bombarding thorium with neutrons which might be protoactinium, but had about a 25-day half-life. When fission was discovered, people said, oh, that's probably the same as that zirconium radioactivity of about 25-day half-life. So Seaborg said, "Why don'tyou start looking to see if that really is protoactinium or zirconium?" Because if it is protoactinium and it's a beta emitter it'll decay into uranium-233, which might be the first member of one radioactive series that didn't exist, the so-called 4N+1. You divide the mass number by four and add one. We had the other series that did exist. So I started working on that, to try to find out if that radioactivity were really protoactinium.

So there I was, the first semester at Berkeley, taking the courses and teaching Chem 1A as a teaching assistant--and, by the way, learning

Chem 1A because Berkeley chemistry was a lot more sophisticated than Oberlin's.

Hughes: How were you doing in Oppenheimer's class?

Gofman: Terrible. After about two weeks, English and Duffield and I--we hadn't talked about it--sat down together and asked each other, "Do you understand anything here?" None of us really understood anything, so we all dropped out. It was a wise decision because we just didn't understand anything.

Hughes: Was he talking above you?

Gofman: I think really the situation--to be perfectly honest, we were not really prepared. But I think even if we were prepared, I think Oppenheimer fundamentally talks to a small group of people who are on a different wavelength from most people. I think if I had studied hard for three years I probably wouldn't have done much better. I just think I wasn't on that wavelength--although I had an interesting experience with Oppenheimer later in the course of the work. At any rate, I started trying to do research in the lab and had to learn a lot about the chemistry of protoactinium.

[tape interrupted]

Gofman: ...and I was taking my courses. There was Leo Brewer, who by the way about three weeks after he came did take his prelims and he did pass, which was even more discouraging. I really felt in that first month, first two months really, that I had bitten off very much more than I could chew. The

Oppenheimer experience discouraged me because I just thought it was my deficiency, totally. And Brewer just didn't even have to take the courses. And then trying to learn Chem 1A so I could be an effective teaching assistant. Oberlin was really small-time by comparison. So I felt a little bit ill at ease in the big-league school. But I managed to hold it together.

I didn't do brilliantly in my courses in thermodynamics, but I did well enough, I guess. I don't remember if I got an A or B+. And I took a course in advanced inorganic chemistry with Professor [William] Bray, now dead. It was a good course on inorganic chemistry; taught me a lot. I began to know Wendell Latimer, who later became the dean of the College of Chemistry, because he had a lot to do with the teaching of Chem 1A. I was very much impressed with this man's mental agility. I managed to get through the first semester, although as I say I was really concerned that I was in over my head, and even got started with my research.

Research with Glenn Seaborg

During the Christmas vacation I had gotten the experiment to the point where I was able to prove that that was protoactinium and not zirconium and we wrote it up, Seaborg and Joe Kennedy. Kennedy was a young instructor working with Seaborg, who later headed the Los Alamos chemistry group under Oppenheimer and then went back after that to head the chemistry department at Washington University in St. Louis and died at 38 years of age of stomach cancer. He was one of the finest chemists I ever saw. He just had magic hands in a chemistry laboratory, seemed he could do anything. We published that paper in the Physical Review.

Knowing that that was protoactinium decaying by a beta particle, that it was going to go on to uranium-233, Kennedy had built some equipment with which we could look for alpha particle activity growing out of a beta emitter. It's a technically difficult problem because the alphas give a very powerful burst but if enough of the beta particles add up, they can give a burst like the alphas. So when you're trying to look for a few alpha particles occurring amidst thousands of betas, it's very difficult. But Kennedy built an analyzer which might have been able to do the job, so we decided we'd try.

At that time I was getting a lot of help in my laboratory education from Seaborg's first graduate student, who had come the year before. Seaborg was just a young instructor. He'd had one graduate student before our group, when English and I were both working with him.

[interruption]

He had Arthur Wahl working on a problem, the chemistry of element 93. They had just discovered 94. Wahl was doing the chemistry. Taught me a lot of what he had learned in his first year here. He was a year ahead of me, helped a lot.

We did a preliminary experiment which was very tantalizing, suggested maybe we were seeing alpha particles from uranium-233, but it was just too close to background.... You won't believe this, but I wanted to stay for the summer working on that problem, but Seaborg could not get the funds to support me for that summer, even at the $60 a month that I was getting with my teaching assistantship. There were no government funds and things like that. So I managed to get a teaching assistantship for the first six weeks. They had one opening and I taught Physical Chemistry 111 lab with Professor Axel Olson. It was a good education. I got to work on my research then. Then the last six or seven weeks of the summer there was no way to support ourselves. So we got a ride with some lady who wanted some co-drivers back to Ohio and stayed with the family, because rent was too high in Berkeley--too high, $35 a month. But if you don't have it, you don't have it.

Hughes: And your wife wasn't working?

Gofman: She wasn't at the beginning, but she got a teaching assistantship the next year in the anatomy department. She was a zoologist. She was taking anatomy, thinking of working toward a Ph.D. in anatomy. The next year she did get a teaching assistantship. [We] came back in the fall, the second year at Berkeley. I had my teaching assistantship but by then there was already the concern over the war situation. We weren't in it yet, but we were near.

Hughes: Were you thinking at all of any connection there between what you were doing and the war?

Gofman: About the end of the first year that I was there, they [held] these talks between [Ernest O.] Lawrence and other scientists--in fact maybe now they can be published--when they were talking about this work. When I got back in the fall, there was enough concern about that that Seaborg said, "You know, really we'd like to move a little faster on these things. Maybe I could get you out of all your teaching assistantship duties and do some limited amount of teaching." As a matter of fact he arranged with Latimer that I would read problem sets and exam papers for Professor Giauque in his course in what's called 216H. It was a graduate course in thermodynamics. I had managed to pass Professor Giauque's course. I think I got a B+ or an A, can't remember. I wasn't outstanding.

But I really learned a lot that fall reading, as I took the course again, went to the lectures. Interestingly enough, Luke Steiner took a sabbatical and came to Berkeley and he wanted to see how Giauque taught that course. So he and I took that course together, an interesting coincidence. I graded the papers and the exams, but I had to learn a hell of a lot about that course. I am amazed at how much [more] I learned on becoming a paper reader than what I'd learned when I took the course just a seme~~m~~ster before. So that gave me a lot of time on my research.

A few funds were coming in from some national sources to Seaborg. I got a bigger bombardment of thorium nitrate and I isolated uranium-233. In fact, in the course of that work, even more funds came from the Office of Scientific Research and Development and Seaborg hired a couple of chemists. One of them, Ray Stoughton, who had a Ph.D., was assigned to help me with some of the last stages of the large bombardment. There was the problem of getting the protoactinium out of the bombarded thorium. There was no problem of proving that it was decaying and producing alpha particles, because we had a sample maybe about twenty times as big as what I had when the tantalizing possible alpha particles had been observed in the first year of that work at Berkeley.

Hughes: Tell me your impressions of Seaborg when you were a graduate student, if you can separate that from what came later.

Gofman: Well, I was in the lab with Arthur Wahl, who was working with Seaborg. Seaborg was then going with Helen Griggs, who was Ernest Lawrence's chief secretary. Very early, Arthur Wahl and Spoff English and I decided that Seaborg was just a very, very great entrepreneur and a real manipulator. Seaborg hung up his lab coat when Arthur Wahl came there and I never saw Seaborg put a test tube in his hands in all my time at Berkeley, not once. I got there in 1940. Arthur Wahl had come in '39. But Seaborg was intellectually very sharp. He knew the problems and he had very helpful suggestions as to what you might do.

Hughes: So he was really keeping his finger on what was going on?

Gofman: Oh, yes. He wasn't just an administrator. He not only kept his finger on it, but he was going with Helen Griggs. So the way it turned out, at about 10 o'clock at night we'd have our studying done and everything and we'd start doing our research. Gilman Hall on the Berkeley campus from about 10 o'clock at night to about 5 in the morning was like Grand Central Station, all kinds of people working on their research because that's when you worked. About 11:30 at night, after his date with Helen Griggs, Seaborg would show up and drop in the lab and ask Art Wahl what was new and what he did that day. We finally sort of got irritated about this because--you know how it is in a lab--sometimes nothing had happened that day or you had messed something up.

Hughes: And you didn't want to talk about it....

Gofman: And we weren't terribly anxious to talk; we were anxious to work. He would stand on one foot and then the other, waiting to be given some tidbits. He didn't really want to talk about the work, just "What's new?" We sort of felt a little bit pressed upon.

He was really very friendly to us, helpful, and as I say, often had a very good idea, and facilitated things. We needed bombardments; he did all the arranging with Ernest Lawrence, and facilitated getting me off of my extra teaching, to just do that one course with Giauque. He did all that. He was a good arranger.

Hughes: Was he on good terms with the rest of the chemistry department?

Gofman: So far as I could tell at that time, he was, yes. I didn't notice anything. Joe Kennedy worked with him. Joe was also a young instructor and then Joe broke away because he wanted to work for himself. Joe had helped make some of the equipment that I had got to use for my problem. At any rate, Seaborg was then beginning to deal with some of the national people. Do you have a copy of that Seaborg article on the early days of heavy isotope research? [Early History of Heavy Isotope Research at Berkeley, August 1940 to April 1942 (Lawrence

Berkeley Laboratory, University of California, Berkeley, June 1976)]

Hughes: No.

Gofman: Well, I'll give you one before you go. He describes the day-by-day workings of Wahl and me and others and his own things. It's sort of a history of what actually happened in the early years. He just wrote it a couple of years ago. After all the controversy about radiation and cancer, I was over at his house working on his history of heavy isotope research at Berkeley.

So I did complete the work to prove that I had uranium-233 and I measured the half-life of it. Now you have to understand that I had in existence four micrograms of uranium-233, four one-millionths of a gram. So I never saw the uranium-233. It was isolated from 50 pounds of thorium nitrate, carried through to the protoactinium stage; then I let the protoactinium decay. Then I had the problem of isolating this minuscule amount of uranium from all that solution, and isolating it pure and free of any other uranium. I had to clean up the protoactinium of other uranium, because otherwise the other uranium would go with the U-233. [I] worked out the technique for doing it and finally was able to plate that uranium onto a small copper plate and measure it to see whether it would fission with neutrons. We proved that it would fission with both slow and fast neutrons, indeed that it had a better cross-section for fission than plutonium.

By the way, interestingly enough, which I'm rather proud of, both the fission cross-section measurements, made on an amount that I had never seen, and the half-life measurement were neither of them off more than 20 percent when they later had pounds of it. That was an interesting era when you did all the chemistry without ever seeing the thing you were working with.

Hughes: This would have been the 60-inch cyclotron?

Gofman: Yes, the 60-inch cyclotron. We were using neutrons mostly. They had the beryllium target, where the deuterons hit the beryllium and then the neutrons came out all over. We'd stack our samples right up near the target to capture the neutrons. I did have one bombardment of thorium with deuterons, just a sort of side thing to try to see if I could find protoactinium-232 and uranium-232, both of which we did discover from that bombardment. So I discovered four nuclides, protoactinium-232 and -233 and uranium-232 and uranium-233. The uranium-233 now stands as one of three things you can make atom bombs out of, or nuclear power. One version of the future proposed breeder reactors is to use thorium-232 to make uranium-233.

Hughes: But this was in the future. You didn't make those connections at that stage....

Gofman: No, the importance with respect to bombs was already known. I wasn't very much into the physics of that whole part of it.

Hughes: Did you have clearance at that stage?

Gofman: It was just about in that period when the whole Manhattan District came in and all the clearance thing did start, yes. When I left the project later for medical school, I had to give up my notebooks because they were secret.

Hughes: Where are those notebooks?

Gofman: I have them now. They gave them back to me after they were declassified. The clearance thing came up in... I'm a little hazy on the '41 or '42 period right now.

Hughes: I should be able to help you with that. Was it February '41 or '42 that Seaborg goes to Chicago?

Gofman: It was February '42. Do you know the story on that whole thing?

Hughes: No. I want to hear it from you. I want to know why you didn't go, for one thing.

Gofman: Well, I'll tell you, that was an interesting thing. It must have been February '42, but by then the Manhattan Engineering District had moved in and we had money and facilities.

Hughes: And more people, too.

Gofman: And more people, yes--quite a few people were working on it. Arthur Wahl was finishing his Ph.D. in '42 and the project had already started. It had been decided by Ernest Lawrence, Arthur Compton, and...

Hughes: Vannevar Bush?

Gofman: Well, Bush was one of them. Oh, I don't see how his name has escaped me--the heavy-water man from Columbia. I know him very well.

Hughes: Urey.

Gofman: Urey, Harold Urey. Harold was at Columbia, Urey and Dunning. They had decided at Columbia they would work on the diffusion technique to try to isolate U-235 with just brute force. Ernest Lawrence would try to isolate uranium-235 by electromagnetic separation. And Arthur Compton and Fermi would see if they could get a reactor to work, and if so, make plutonium as a third approach.

Hughes: And you knew that?

Gofman: We knew all that, yes. So the Chicago project under Compton had brought Frank Spedding, a professor from the University of Iowa, to Chicago to head chemistry. Spedding's initiation into that project was that they needed some purified uranium to even try to make the reactor pile for Fermi. They had some metallurgical skill at the University of Iowa and they succeeded in making some pretty pure uranium. So Spedding having headed that went off to Chicago to be the head of chemistry in the plutonium project that was forming in the Metallurgical Laboratory at Chicago.

Spedding came out to Berkeley just about the time Art was finishing his Ph.D. I wasn't quite finished then; I wasn't too far behind. Spedding asked Arthur Wahl if he would come to--oh, first

Arthur Compton had asked Seaborg to come to Chicago. Seaborg had refused, said he was much too busy in Berkeley. Here he had this empire in Berkeley which was a known thing and he didn't want to go off to Chicago. So Spedding came out to Berkeley and he had heard about this guy Arthur Wahl who was working with Seaborg. In fact, he had done all the chemistry of plutonium and element 93, and the world's knowledge of the chemistry of elements 93 and 94 on a practical basis was in Arthur Wahl's head.

After I finished my uranium-233 work, I started working with Art Wahl, learning the chemistry of plutonium. But he was it, in the world. He had been working under Seaborg, but Seaborg never dipped his hands in the lab. I don't think he could have isolated plutonium the way Art Wahl could and I could later.

So Spedding said to Wahl, we've got to have somebody come to Chicago that knows the chemistry of plutonium; would you come? You can have the plutonium chemistry section. So Art said that would be fine, he was delighted. As a matter of fact, Bob Duffield and Spoff English and I had a big party celebrating Art's appointment as the head of this project in Chicago and the fact that he was getting his degree and he was going to go off.

So I was working along, starting to learn the chemistry of plutonium, because Art was going to leave and we wanted to keep that going there. There was a lot to do on the chemistry of plutonium. We only knew a little bit about it. One day Art came into the lab with a long face and a lot of four-letter words and told us the story. He said, "Guess what? Seaborg has decided to go to Chicago and head the chemistry project and has told me how delighted he would be to have me come and work with him there." So here Art, having been promised the headship, is now what he felt was almost back to being a graduate student under Seaborg. So we said, "Are you going to go?" He said, "Hell, I'm not going at all." And Seaborg was quite upset about that, that Art wouldn't go. But Seaborg had cased the situation, said that's where the opportunity is, and he was shortly gone to head the thing at Chicago.

He didn't get along well at Chicago. I can tell you some things about that because we used to have meetings at the Metallurgical Lab in Chicago every four to six weeks, of the various people working on some aspect of it. When I went there, some of the other departments would approach me and say, "We've got to do something about this guy Seaborg." They just felt he was an empire builder.

Hughes: So it was personal, not scientific.

Gofman: I don't think anybody could ever say anything unfavorable about Seaborg scientifically. They felt personally that he was an empire builder, that they were getting short shrift. They were just mad in Chicago about him. I know because I used to go there as an outsider and they'd say, "You've got to help us do something about this guy Seaborg." Well, I wasn't much of a wheel to do much about it.

By then I was working on many aspects of plutonium chemistry, studying it and studying the possible ways you could isolate it from uranium and fission products. Art was the nominal head of our group. He stayed in Berkeley. All the work was under the faculty guidance of this man Wendell Latimer, whom I liked very well. He was just a hell of a good chemist. Weekly or every two weeks we'd get together and talk about the research. Wendell Latimer had a lot of good ideas, really a very bright guy.

Then Art said one day that they [had] decided to really start working on making the bomb, and told me about the Los Alamos project, that Oppenheimer was going to take a group down there and that Joe Kennedy was going to head that group and that he, Art Wahl, was going to go, and that everybody in the Berkeley group was invited to go: of the leading guys, English--no, English had already gone off to work with Seaborg in Chicago--but Duffield, Art Wahl, and I. Then they had a fellow named Cliff Garner who had come to join us. He had been a professor at Texas but came over to the war work.

We were invited to go to Los Alamos with Kennedy to form the chemistry group. He said, "If you're interested, why don't you talk with Joe Kennedy?" So I talked with Joe and I talked with Oppenheimer. There was only one reason I didn't go. Helen was enrolling in the first year of medical school. She had finally shifted from anatomy to medicine. Oppenheimer said, "I want to tell you about this project. We're going to go. We don't know how long the war's going to last. We will not guarantee that you will be able to communicate with the outside world at all. It is just going to be very, very tight security. You're married. Your wife ought to come with you or you do face the possibility that you won't even be able to communicate with her." That seemed rather drastic, but in the times it was regarded as reasonable.

So we talked about it and Helen thought she really wanted to go on in medical school. So I said, "Well, I don't have to go down to Los Alamos. There's plenty of work that can be done at Berkeley." So I went back and told Oppenheimer that I would stay in Berkeley [and] I'd be happy to cooperate.

So Art and Joe Kennedy and Duffield and Garner all did go to Los Alamos with Oppenheimer, and I became the head of the residual group working on plutonium. We had about 15, 18 people still there [with] less training than the rest of us, and Latimer was the faculty head. I was responsible to Latimer. I got my degree somewhere during that period, I remember. The third year of my graduate work, I didn't have to do any teaching at all because I got one of those Rosenburg scholarships, which was $1,200 and no work. So that was fine. In fact, I gave up that fellowship during the third year because I was able to get on the payroll of the [Manhattan] Project at that time. I was already finished with my degree so there was no point in staying. I finished in two and a half years. Somewhere in there during the first year I did pass my prelims, although I thought I would just squeak through.

Hughes: Your ego was reestablished.

Gofman: Slowly. The first year was very, very slow getting any ego reassurance.

Hughes: Even with the uranium work?

Gofman: Well, the work in the lab went well, but there was all this background stuff that I didn't know. Just felt there was a big underpinning I didn't have. Teaching Chemistry 1A was a big help in getting a better feeling about things. Yes, things got better. I felt better towards the end of the first year, but I still felt it was big-league. But the research did go okay.

Plutonium for the Manhattan Project

Latimer said that Professor Oppenheimer was coming in and he wanted to talk to him and would I come down. So I came down to Latimer's office and Oppenheimer came in. Art Wahl had by then isolated at a maximum about a twentieth of a milligram of plutonium. That was the world's supply. Oppenheimer said, "We need about a half a milligram of plutonium." I said, "Well, at the Oak Ridge reactor you're going to have grams in six months or a year. So what do you need?" He said, "We have some crucial measurements. We need a half milligram in weeks, and weeks count." Everybody felt that it did in the war. So I said, "Well, what do you want?" He said, "Art Wahl and Joe Kennedy say that you can make it for us."

I had by then worked out some tentative processes by which one could isolate plutonium from uranium, on a laboratory scale in beakers. So I said, "Well, what do you have in mind?" and he said, "Well, we estimated that it would take roughly a ton of uranium bombarded for a couple of months and you could get maybe a half-milligram of plutonium." So I went through the arithmetic and it looked about right. I said, "Well, who's going to let us stack all that stuff around the Berkeley cyclotron? We'd have to have every inch of space that's available there and they'd have to run the cyclotron night and day for us." "It's all arranged. I've talked with Ernest about it. All we need is your assurance that you'll do the chemical work." I said yes.

Hughes: This is about 1943, isn't it?

Gofman: Yes, very early. I said, "You know, I've only done this process on a beaker level. I don't know if it will work scaling it up to a ton of uranyl nitrate.

Hughes: There is a difference....

Gofman: Yes. So he said, "Well, [the Los Alamos group] can't do it. They've got to stay at Los Alamos. You're the only one who can do it, so will you do it?" So I asked Latimer if that was what he felt we ought to do and he said, "Yes, I think we ought to do it." So we agreed.

So the Berkeley cyclotron ran night and day for about seven weeks bombarding our ton of uranyl nitrate. That's not a small amount of material. Room 110, the old chemical engineering laboratory of Merle Randall, who is the other name on Lewis and Randall's Thermodynamics, professor of chemical engineering--it was his lab, but he wasn't using it, so we set it up in vats. Bob Connick, who is now one of the deans of the school--he's been chairman of the College of Chemistry--had joined our project. So he was working with me on it. We set it up in batches of ten pounds at a time, dissolving it in great big Pyrex jars. We set up a night-and-day operation with about ten or twelve people working, not under safe conditions at all. But we didn't know enough....

Hughes: You didn't know?

Gofman: Didn't know enough, no--dumbbells, really dumb. It was a dumb operation. We made a concession to safety by putting a piece of lead around the Pyrex, but we were always looking in.

Hughes: John Lawrence wasn't enough in your life to think of perhaps consulting him about the biological effects?

Gofman: No, John was there. I knew he was over there in the Crocker Laboratory, but it just didn't occur to me to consult. Worse yet, Joe Hamilton was there in Crocker Lab. He died of a leukemia which undoubtedly was associated with the work. Joe was a physician and he handled things in such a sloppy manner that I think I just sort of got the idea that, gee, we do things so much more cleanly than Joe and if he's a doctor, it must not be that...

Hughes: Must be all right.

Gofman: Yes. So we handled things there ten pounds at a time because they needed it. We could have made the job much less hazardous if we had let it cool another month so all the short-lived radioactivities would die away. But time was of the essence, so we handled it not too long after it came off the cyclotron. We all got a good dose; I don't know how much.

I have to tell you something funny about safety and health. Stafford Warren, who later became the founder and dean of UCLA med school, was the chief health officer of the Manhattan Project. Somewhere they had heard that we had this operation going on. So he came to inspect this little chemical engineering operation--the isolation of plutonium. Well, we had to use sodium nitrate and sodium acetate in five-pound boxes; that's how it came packaged from the Mallinckrodt chemical company. The process was called the sodium uranyl acetate process, which I devised and patented under the Manhattan Project, for isolating plutonium. We were going to use several tons of sodium nitrate and sodium acetate.

So as we dumped these cartons out, we'd just stack them up down in one end of the lab. The only comment that Stafford Warren made about the safety of this operation--here we were handling red-hot uranium nitrate--was that he was worried about those empty sodium nitrate cartons hitting somebody on the head. That was his only recommendation: to get those cartons down. So he didn't know too much either and he was the radiologist in charge. Then he went off and we never saw him again.

Well, it was one of those things where just everything worked. In about three weeks, we had taken that stuff down from a ton of uranyl nitrate to plutonium, about half a cc of liquid. All the processes worked. We had no blunders. We had very small losses and we finally found that we had not half a milligram of plutonium, but 1.2 milligrams. We had gotten about 2.4 times as much.

Oppenheimer and Kennedy came up and they took 750 micrograms--that was about two thirds of it--and left us a third to work with on the chemistry.

They said that'd be enough. They took it down to Los Alamos. Art Wahl and Bob Duffield were going to do some further work with the plutonium and a tube broke in the centrifuge and they were mopping up all that stuff. That had it back up into liters of solution after we'd given it to them in [micrograms]. But they managed to get it back. We were just lucky no centrifuge tubes broke.

Hughes: It must have been a little bit later on, after the pile was really operating, you received a sample--I think it was one of the very first samples of plutonium for biological studies.

Gofman: That was after they had appreciable amounts....

Hughes: Was [plutonium] being farmed out to all sorts of people at that stage, or was Hamilton the logical person...?

Gofman: He had begun the question of where would the various radioactivities go in biological tissue. A woman by the name of Dorothy Axelrod was working with Hamilton on that project, and later Pat Durbin joined them.

Hughes: Yes. That was after the war that Pat Durbin came in.

Gofman: Yes.

Hughes: So it wasn't any particular Berkeley connection. It was just that Hamilton was the logical one to do it.

Gofman: No, Hamilton was the guy that was doing the work on if you inject any one of the fission products or plutonium into animals, how much will stay, because they were beginning to be concerned.

I have to tell you an interesting thing that happened. It was '41 or maybe early '42. Art Wahl and I were working one night and I said, "You know, Art, if a bomb is successfully made, if you figure out the amount of radioactivity that will be generated and the fission products, that's a hell of a biological hazard because it descends to the earth and it'll get into all kinds of foods and people will breathe it. It's going to be an [enormous] amount of radioactivity because you just scale up how much it would be." And he said, "Oh, they've got that figured out because when the bomb goes off, all that stuff's going up into the stratosphere and is never coming back." That was an interesting error that they had and you know it wasn't until the early fifties that they realized that all that stuff was coming back.

Hughes: The fallout was falling.

Gofman: Yes, that's right. They really did assume it was going to stay up in the stratosphere forever. But it was in '41 or early '42 that Art said, "Oh, they've thought about it and it's all going up to the stratosphere and never coming back." And that was the last I thought about it for a long time.

Hughes: And there were never any second thoughts in your mind about to do or not to do the bomb?

Gofman: Well, I've been asked that question sometimes by people. I'll tell you my attitude then: I didn't have any qualms at all because as I looked at it then, and I think I'd look at it again today that way, Nazi Germany was so monstrous. They were the ones we thought we were making the bomb against because there was already the rumor that they might be working on this. There was all that talk about the heavy water in Norway. The idea of making a bomb that would possibly win the war against Nazi Germany just seemed to me very worthwhile. So there were no qualms.

Just to give you an idea about that work: I finished my Ph.D. and I was working on this project and Helen was in medical school, but it was no different than during the graduate-work days. We were there until 2, 3, 4 in the morning, probably worked about 18 hours a day many days on that and worked quite enthusiastically. And it seemed important to do. So I didn't have any qualms then because of what I considered the monstrosity of the Nazi regime.

Hughes: Well, that was true pretty much of everybody, wasn't it? It was just something you did because it was so pressingly necessary.

Gofman: Yes, I can't remember anybody who seemed to express a reservation of any sort about it. The picture of what an atom bomb would mean in terms of Hiroshima and Nagasaki wasn't clear. You thought of it as a bomb to win the war. For example, I don't remember anybody just saying, "We're going to wipe out a city or two."

Contacts with Crocker Laboratory

Hughes: What about other contacts at that stage, outside the little research group--for example, when you went to Crocker? Was that just a business arrangement or did you really get to know the people there?

Gofman: Not too well, really, socially not much. Dorothy Axelrod, who was Joe Hamilton's chief assistant, was being dated by Bob Duffield, who was one of my co-graduate students at that time. So Helen and I went out with Duffield and her several times. We once went to the mountains together.

But I didn't get to know Joe Hamilton well except for arrangements. Joe was in charge of the 60-inch cyclotron and if I was going to bombard a thorium target, I had to make the arrangements through Joe. And there were little things. Sometimes he'd need some help on some chemical thing and he'd come over and I'd work with him. But I didn't get to know him socially. I think I had been introduced to John Lawrence. That was the extent of my acquaintance. I'd met Ernest more, but not John.

Hughes: You mentioned one of the reasons that you chose to work with Seaborg, or perhaps the reason, was because of the potential for tracer work. But you never made that leap.

Gofman: No, I never went over to try to work with them. They were working on using radioactive sodium to trace circulation and they were using radioactive iodine and John was beginning his work with radioactive phosphorus, but I...

Hughes: No contact?

Gofman: I didn't make a contact at all, or didn't try to.

Hughes: What about Martin Kamen?

Gofman: I knew Martin quite well because he was the chemist in the Crocker Laboratory. Martin was like Joe in his carelessness in those days. When Martin came into our lab, as he often did come over for one thing or another chemically, he could make our [Geiger] counters sing out from radioactivity on his coat. I knew Martin and I knew Sam Rubin, who was working with him. Sam was this very talented chemist who worked with him in the earliest days of the carbon tracer work on photosynthesis, the program that [Melvin] Calvin later took over. I knew Martin pretty well.

You asked about the social: There wasn't much social contact because we were too busy. I just didn't do anything much. Duffield, who was in the department, and English, these co-graduate students, we used to do things together. We went to the mountains together and things like that. Duffield and I and a fellow by the name of Wayne Wilmarth, my third year, each became one-third owners in a 16-foot sailboat. That was when I was really moving up into the dollars bracket; it was $430. We went sailing quite a bit in Berkeley.

Ernest Lawrence

Hughes: Tell me a little bit about Ernest Lawrence.

Gofman: Not too many contacts during the period of working on the uranium-233. He knew about that work and then that I had done the plutonium job. So I got to know him rather briefly. He was very friendly. One of the things about him was that he had just an enormous eagerness to know what you're doing, learn what you're doing. Just wanted to know. He had a very curious manner, almost a delight in hearing about what somebody's doing. So he knew about my work, but I didn't know him too well or see him too much.

Hughes: Did you ever get the impression that he knew at least the general outlines of that whole empire that he was running, particularly during the war?

Gofman: Oh yes, very definitely, very definitely.

Hughes: And that was by personal intervention or, do you think, more secondary information?

Gofman: I think it was personal because I had a later experience which showed me how Ernest Lawrence worked. When I came back [after medical school] apparently John had talked to Ernest. Ernest knew about my previous association with the Project. After medical school, they gave me the assistant professorship. I was working on lipoproteins.

One time I went up on the Hill to give a talk on the ultracentrifuge and what we were doing. The analytical ultracentrifuge has an optical system. It's based on what's called shadow optics or

Schlieren optics, which gives you these pictures from which we measured the various lipoproteins. I gave this talk on the Hill to physicists and chemists. This was about 1950. I'd been there two and a half, three years. I figured, if I start explaining this optical system to the physicists, it would be an insult because they probably understand this optics much better than I do. So I said, "We use the Schlieren optical system. It gives these diagrams from which we..."

I got through the talk and Ernest Lawrence [said], "A very interesting talk, John. I don't understand the optical system." So I started trying to put it on the board and wrote the equations and showed him. He says, "I still have some problems with the optical system. I'll come down and see it sometime. Let's not take more time now." Several revelations from that: first, Ernest Lawrence wanted to understand everything; second, here was this great, prominent scientist, willing, in the presence of a few hundred of the staff members, to say "I don't understand this." The others might not have understood it either but they'd never say so. He said it.

Well, I got through the talk, never thought more about it. About two weeks after that, at about 5:30 in the afternoon, I was sitting in the lab at a desk working. Ernest Lawrence poked his head in and said, "Have you got a little time?" I said of course. He said, "I'd like to understand that optical system." And so we went into the centrifuge room and we took off all the tops of the gear and went through it step by step. As soon as he saw the gadgetry he said, "Oh, fine, I've got it." We spent about an hour and a half and then he left.

Hughes: Really followed through.

Gofman: You bet. One of the things about Ernest, he wanted to know what you were doing, how you thought, but boy, once he became convinced you were a serious and worthwhile scientist, then he would just back you in any way you needed. Ernest Lawrence just opened all kinds of doors and pushed aside all kinds of obstacles in my lipoprotein work--not my original work on uranium, but in this.

Medical School at UCSF

Hughes: After the war, you went to medical school in San Francisco.

Gofman: Actually, when the war was still on.

Hughes: Can you tell me how that came about?

Gofman: Yes. Let's see, Helen had already gotten into medical school. She had gotten in at the time we had elected not to go to Los Alamos. So I did that job on the plutonium for Oppenheimer and continued to do work toward developing better and better processes for separating plutonium from the presumed uranium and fission-product mixture that was going to come out of the Hanford reactors. And I remained in contact with the Metallurgical Lab project of Arthur Compton. I used to go back every four to six weeks to Chicago or to Oak Ridge because Oak Ridge became the center for all three of the projects to have a semi-works or the actual works there. For the plutonium projects it was semi-works. I worked both at Chicago and at Oak

Ridge on those visits with the DuPont engineers who were trying to scale up the laboratory project to full size.

Then it just occurred to me that no one knows how long this is all going to go on, but the next phase of all this was just all chemical engineering work, really. Having started out thinking all science was engineering, by this point I was very much more of a purist in thinking chemical engineering is just sort of large-scale cookery and I don't want to get into it. So I thought, well, as far as the ability to get the plutonium out, there's already enough known about that, and I had elected not to go to Los Alamos. So I said, I think this is a time when I could elect to go back to medical school. Nobody knew at that time, at the beginning of '44, when and if the war would be over. I figured I'd go on into one of the services after getting a medical degree.

So I talked with Latimer and said I thought the Project really didn't need me that much any more, since it was all getting to be just engineering for Hanford, and I'd like to go back to medical school. He said, "If that's what you want to do that's fine with me. I could arrange for you to have an instructorship in the department of chemistry. I think you'd have a good future in the department if you wanted to go that route." I said, well, I appreciated that very much, but that I thought I wanted to finish the medical work. So he said fine, and I told him I would apply to medical school.

So then I had this choice: Should I now go back to Reserve and try to take up [after] my leave of absence, since Helen was by then in the second year of medical school here? So I thought, well, that's sort of absurd, either split up for that time or have her try to transfer. So instead of contacting Reserve I came over to see Francis Smyth, who was the dean of the medical school here. By the way, he was the brother-in-law of Robert Stone.

Hughes: Oh, I didn't know that.

Gofman: Yes. Dr. Smyth was very cordial. I explained that I'd had the first year of medical school and had gone off to study chemistry and I'd like to apply for the second year. I thought I could get back into Reserve on the leave-of-absence basis, but I'd prefer to go to Cal. And so he said, "I think there's a good possibility. We do have a limited number of places in the second year, but there are always some dropouts." And I did get admitted.

I had to make up one thing. Physiology is taught in the first year of medical school here. It was taught in the second year at Reserve, so I hadn't had the physiology, but I had had some courses that they hadn't had. So I had one semester that I stayed working on the Manhattan Project and took physiology at Berkeley to make that up. Then I entered the second-year class in '44. We were on the accelerated schedule. I joined the Navy V-12 program because you had to be in one of the military programs in order not to get drafted, even as a medical student. They wouldn't give you a deferment. In the Manhattan Project, I would have been deferred forever.

Hughes: V-12 signifies the medical...?

Gofman: The name of the Navy medical program. When the war in Europe was over and I think before the war in Japan was over--no, the war in Japan was over too --the Navy immediately discharged me. They didn't want the medical students; [neither] did the Army. So the last year of medical school was on our own; we weren't in the service. We were not obligated. At that point they didn't want us. They didn't want to finish supporting the people in medical school and they didn't have any required obligational service. Later they did reintroduce the medical draft. So I finished up at UC medical school, the last three years, part of it in the Navy, part of it not, and didn't go into the service. I interned in internal medicine at the University of California Hospital.

Hughes: Why did you...?

Gofman: Why internal medicine? That was interesting. I thought that the place where you would do research on major diseases, approaching [them] chemically, seemed to be in the field of internal medicine because all the metabolic diseases, diabetes, endocrinology, heart disease, all fell in internal medicine. So that is why I chose it, though that is really sort of silly because there's good chemical research being done in surgery departments and pathology departments and pediatric departments. But I chose internal medicine for that reason.

The question you ask is sort of interesting because Howard Naffziger was the chairman of surgery at UC, was a professor of neurosurgery, a dominant figure in the medical school of that time, the forties, when I was there. I did quite well in surgery and he called me into his office one day and he said, "What are you thinking of interning in?" I said, "Well, I thought to intern in internal medicine." He said, "What a shame, that's a dying field. Everything will be done surgically in the future." He just thought it was a waste of a good man to go into internal medicine. But I did nevertheless.

I was not thinking of going into the practice of medicine. I thought I wanted to do at least an internship, but probably not more. That's why I elected to take that one-year internship at UC and see what there was available in the way of positions. How in the world...? Oh, I somehow contacted Hardin Jones. I don't really remember how I got in touch with him, come to think of it.

Hughes: Not John Lawrence? At the time he was an assistant professor in the department of medicine.

Gofman: Yes, but he didn't come up over here [San Francisco] very much. [The appointment] was formalistic. I didn't contact John Lawrence. How in the world...? Maybe I did write John a letter, but somehow I got in touch with the Division of Medical Physics.

But at any rate, I got to talk to Hardin. I had one quarter free that I could do a clerkship. You could be in either the surgery department or the medical department. I asked the medical school if it would be all right if I went over and did it in Donner Lab. They said okay.

I worked with Hardin Jones and Ernie Dobson and Lola Kelly. They were working on chromium phosphate as a way of measuring liver circulation, an idea of Hardin Jones', and it was a good idea. I worked on trying to make better colloidal chromium phosphate and some other colloids, which I did. I worked that quarter and then came back to my internship. But then during the internship I used to go over, when I wasn't on call, on Saturday and Sunday and work in Donner Lab on that problem.

Job Opportunities

Meanwhile, I met John Lawrence and I applied for a position in the University of California. They said they would see about trying to get me an assistant professorship. I don't think there was a question of an instructorship, it was an assistant professorship. About May of the [year] I was about to finish, I did have a fairly firm offer of an assistant professorship in Berkeley. John really didn't know me very well, but I had gotten to know Hardin, working with him on that project, and Ernest of course knew me from the days before. Of course they consulted Ernest, and they said there would probably be an assistant professorship, but it wasn't absolutely positive.

Joe Kennedy, this man I told you [about], this excellent chemist who worked with Seaborg and then went to head the Los Alamos lab, had then gone on to become head of chemistry at Washington University in St. Louis. It was interesting how things came together. Arthur Compton, after the war, became the chancellor of Washington University. Art Wahl, Dave Lipkin, Sam Weissman, and all those people came up from Los Alamos to work at Washington U., and Martin Kamen had gone to the department of radiology to do research there.

So I contacted Art Wahl about the fact that I was seeking a position. He talked to Martin and Martin talked to the head of the radiology department, a fellow by the name of Wendell Scott. The other thing I thought of was the possibility that the Mayo Clinic might have some use for me. So I wrote a letter to the Mayos telling them about my background and that I would like to see them about a position where I might spend part time working toward the clinical qualification in medicine or a research position.

There was one more possibility, at Portland, the University of Oregon [Medical School]. Edwin Osgood, who was working on hematology, had met me when I was down at Donner during that summer. He had a project on bone marrow culture and he was looking for somebody to join him. So he contacted me about a position. I had some vacation time coming, so I went to Portland and saw Osgood and his project, planning to go on to the Mayo and to St. Louis.

I elected while right at Portland to turn down the job with Osgood and the reason was a very simple one. He had a dinner for me and there were a group of Portland businessmen who were funding his work and would have funded my position and they said, "You just do good work and you can have anything you want." That just scared the hell out of me, having a group of businessmen deciding whether my work was good or not. So I told Osgood I liked his project fine but I didn't like the circumstances of the position. I wouldn't take it.

I went on to the Mayo. They made me an excellent offer that I could go on and work toward the boards in internal medicine and work in their research labs. Instead of what they paid their ordinary fellows who were just training, I would have gotten a very decent salary. It was very attractive and the Mayo is an attractive place.

Then I went to Washington University in St. Louis and they offered me an assistant professorship. I would have been working in the same lab with Martin Kamen, and all my friends were there in the chemistry department. Joe [Kennedy] died shortly thereafter, at 38, as I told you. But Martin was there and it was attractive. I would have worked toward the board qualification in radiology and done research. But St. Louis was not attractive to me as a place.

Donner Laboratory: Assistant Professor

So I came back having turned down the offer in Oregon, and the Mayo offer and the Washington University offer were open. I contacted Berkeley again and asked John Lawrence whether I could have a definite word, and I did get a definite offer. I decided that Berkeley was not necessarily better than some of the other offers, because Berkeley had one big drawback, as I saw it, and that was no clinical facility of consequence. They were talking about possibly moving the medical school to Berkeley instead of moving anatomy and physiology to San Francisco and finally the San Francisco campus won out and they moved the Berkeley first year here [San Francisco].

So I had some concern about the absence of a clinical facility. John said, "Well, you could do everything you want. We have a small clinic with bloodwork. I'm sure they'd be happy to give you a position in the medical department [in San Francisco] along with your assistant professorship here. I have a position in the medical department." And indeed they did arrange that; I became an instructor in medicine. I elected to take the Berkeley job. In fact, for two years I did work two days a week at the medical clinic here, teaching.

Hughes: But staying full-time at the medical school was never in your mind?

Gofman: There just didn't seem to be any positions. First of all, if you're going to stay full-time in the medical school as a member of the department of medicine, since they didn't have any big research labs in those days... You look at this place now and you say, gee, there are all kinds of opportunities. There was nothing then. There was just the little UC hospital and the Hooper Foundation and that was about it. I would have had to complete the training in internal medicine. But I knew of no job being offered. William Kerr [chairman of the department of medicine] didn't suggest any.

Hughes: What put the bee in my bonnet, so to speak, is that Raymond Birge has written a history of the physics department--have you seen it?

Gofman: No, I haven't seen it.

Hughes: He has a whole section on Medical Physics. Well, I might as well quote it to you. He said, "Those in authority at San Francisco were exceedingly angry that we had stolen Dr. Gofman away from them. They intimated that we must have used some underhand method." You were unaware of all this?

Gofman: Well, that's interesting. No, I don't think there ever was any theft. They were very jealous of the lipoprotein work over here.

Hughes: But of course that hadn't happened yet.

Gofman: That hadn't happened. Ray Birge must have had it a little wrong. The antipathy toward me because of the lipoprotein work and the jealousy came later, but I don't remember any suggestion that they wanted to have me have a position there at all.

Hughes: Of course, there was antipathy in general between elements in the medical school and Medical Physics in Berkeley. Maybe you were just more or less swept along with that.

Gofman: I think it must have been much more of that, Sally, than anything else, because I at no time ever had any offer of a position in the medical center other than when John Lawrence said, "I think we can arrange to have you appointed there as well." I got a nonpaying appointment in the medical school as an instructor. Then later it was shifted to a lectureship. I still have it. But it's interesting that they never advanced me in title, even on an honorary basis, as I moved up to the full professorship in Berkeley. I'm still a lecturer now in the medical school.

Friction with the Medical School

Hughes: Is now the time to ask you about--

Gofman: Anything.

Hughes: --that unrest which I believe... My understanding of the situation is that it was something that began with Robert Stone and the neutron therapy, which of course preceded your arrival even in California. But it really came to a head after the war. In 1947 Robert Stone wrote a paper saying the neutron therapy had been harmful, that there had been very unfortunate side effects and he wanted no part of it. But underneath it all, from what I gather from talking to various people, was the not too subtle inkling--well, he more or less put the blame on Medical Physics in Berkeley: that they hadn't collimated the machine correctly, that they hadn't allowed the scheduling that he needed for his patients, and so on. This caused a great deal of tension between the two institutions.

Even to this day, there still are remnants of that antagonism, among probably limited groups by now. But I think it's important to discuss from the standpoint of what effect this did have on the development of medical physics at Berkeley, particularly from the standpoint of nuclear medicine.

Gofman: Right. I wasn't in any way involved or aware of the whole neutron therapy thing. I sort of got in after that. I didn't even notice the Stone paper, but I certainly was aware that there was a problem between the medical school and the Medical Physics Division from the time I got there. My appraisal of it and that that I had indirectly when talking with John about it and people like Hardin Jones, was that Robert Stone was very jealous of John Lawrence's existence as an independent entity involved in what looked like a burgeoningly important field. I think their sights were rather parochial and they thought that the University of California wasn't big enough to have radionuclide work on the San Francisco campus and to have John Lawrence as an independent entity in Berkeley.

With the support of the Atomic Energy Commission, as the [Lawrence] Berkeley Lab built up to have a lot of research in not only the medical use of radioisotopes but other things, I think they felt even more threatened and anxious. The radiology department felt threatened and anxious. Robert Stone was a very influential man in the medical center, very influential, and he apparently was quite jealous of John Lawrence. That's my appraisal of the situation.

Hughes: Did Naffziger have any part in this story, do you think?

Gofman: Not that I ever was aware of. No, Naffziger was friendly to me.

Hughes: I rather think you're right. At a considerably later stage, probably in the mid-fifties, when the heavy particle therapy was being contemplated--I mean whether they really would go full blast--I remember Naffziger was one of the people who was brought in to give an opinion. Obviously they wouldn't have invited him if there was an overt antagonism.

When the Division of Medical Physics was set up, and John Lawrence was on the committee and everybody else was from the medical school, one of the stipulations was that any human research or therapy that was to be done on the Berkeley campus had to have the approval of the dean of the medical school.

Gofman: Yes, and I rather suspect they didn't feel that John really checked in with them after that, because he...

Hughes: Well, do you have any recollection of ever--?

Gofman: No, I never recollect checking anything. For two years I worked in Dr. Ernest Faulkner's clinic in hematology here, helping with medical students and seeing patients, and I worked one day a week in John's clinic in Berkeley. But I never remember ever checking anything with the medical center. I think it didn't get checked, and maybe that was what galled them even further, that they didn't have control.

I had a very strange run-in in the course of the heart disease work. In 1950, two years into the heart disease work, I published a paper in Science called "Lipoproteins and Atherosclerosis." I don't mean to interrupt your train of thought, but I'll just tell you this one thing. William Kerr, the chairman of the department of medicine, called me up and said he wanted to come over and see me. So I said fine. I had interned in his department, but he'd never offered me anything or asked me anything. After I finished my internship there, aside from the fact that I had this official appointment as an instructor in medicine and went to Dr. Faulkner's clinic, I never saw Dr. Kerr at all.

I tried to get some blood from patients with coronary disease through the medical department. I needed blood to study the lipoproteins. And I just was getting nowhere, in terms of a runaround on getting patients. I had met a guy by the name of Dr. Thomas Lyon, who was a fellow in cardiology back from the war, the year that I was interning. He had gone down to San Jose and he was still coming out to teach at the medical center. But I got so disgusted with not being able to arrange to get the blood--all kinds of minor blocks. I wasn't getting anywhere, and at that time--well, it's the way I work in research at any time--I decide to do something, I want to do it.

So I called Tom Lyon and said, "You know, we really need to study some patients for coronary disease. Let me tell you about the project." So I went down and he said, "Well, I have a big practice. I could start giving you every patient I have." So we finally ran a series on 100 patients with coronary disease with him, and we published that paper in Science. He and Hardin Jones were coauthors, and Frank Lindgren. I don't think Alex [Nichols] had joined us at that point.

Well, Professor Kerr called up and said he wanted to see me. He came over to see me after the Science paper was published. So it may be that I even contributed to some of the antagonism of the medical school to Medical Physics. But he came in and he said, "I don't understand what you're doing." I said, "Well, I don't understand what you mean, Professor Kerr." He said, "How do you publish a paper on lipoproteins and atherosclerosis in Science without checking it out with the medical department?" I said, "Well, it never occurred to me to check it out with the medical department." He said, "But this whole area of heart disease is within the medical department and nobody publishes papers without checking them out with the department. And you're in the department."

The department hadn't done anything for me except put my name on a list and let me teach in the clinic. I said, "Well, Professor Kerr, I'm an assistant professor in the Division of Medical Physics. There's one thing over here that nobody has ever suggested, that anyone submit something to the department before they publish it. So it never occurred to me that I should submit things to the department of medicine. Furthermore, I wouldn't. I'm very happy to have you see what I publish, but I don't want approval because I don't consider that an appropriate function of the department."

He said, "Well, that's the way we do things." So I said, "Well, Professor Kerr, if it is, you can delete my name from the department of medicine if you wish to." "No, no, that's not necessary," he said, "but you're just not behaving in the fashion of other members of the department of medicine." I said my attitude is that I feel very strongly about freedom of publication and I wouldn't submit my things for approval. So he left and it was on reasonably good terms.

That was the only run-in I ever had, except once, years later, when Ted Althausen was the chairman of medicine. He called me in and said, "I hear disapproving things stated about the things you have to say about heart disease. It reflects back on the medical department." I said, "Well, Professor Althausen, there's just a simple thing to do--take my name off the department of medicine."

The fact is that I had a lot of recognition and it was very much one-way. They were gaining from what reputation I had made in the field, not that I was gaining anything from the department of medicine. And they weren't offering me anything in the way of better cooperation.

Actually during those years between that visit from Kerr and the visit from Althausen, I indeed was very receptive to the idea of collaborating with people in the department of medicine. I did some extensive research with Peter Forsham and Felix Kolb and we published two papers on endocrine disorders and lipoprotein metabolism. I did extensive work with Professor Alex Simon and Dr. Nathan Malamud. We did a large project that culminated in four separate publications on cerebral arteriosclerosis. I did some separate work still with the department of neurology. So I was very, very cooperative. We had things going; it was just these sort of jealousy things at the top.

Maybe I contributed to some of the antagonism, but I had a very strong sense that people were jealous of John Lawrence having this independent show over at Berkeley. What the scuttlebutt said was, well, John can do these things because he's Ernest Lawrence's brother. I think it was an unfortunate thing for [John] to choose Berkeley, in one sense, because of that association. Then, on the other hand, Ernest Lawrence made possible a development that would never have happened otherwise. He was a remarkable man in terms of facilitating things.

The medical school's antagonism was blunted for one very big reason. I don't know how many people know this, whether John ever told you, but the chairman of the Board of Regents during that period, John...

Hughes: Is that Neylan?

Gofman: John Neylan, yes, John Francis Neylan. He was the chairman of the Board of Regents and, man, he was a powerhouse himself. Nobody crossed John Francis Neylan. He thought the world of Ernest Lawrence, and Ernest introduced him to the Medical Physics Division. So in 1950, '51, John Francis Neylan was visiting the Donner Lab and Ernest brought him to see me specifically about the lipoprotein heart disease work. Neylan just thought it was great work, was very friendly, and introduced me to some other people.

As a matter of fact, the one time I've been in the Pacific Union Club in my life, John Francis Neylan had a black-tie party for Loyal Davis, professor of neurosurgery at Illinois. Howard Naffziger was there and I was invited as a guest. As I said, it was my one time in the Pacific Union Club. So I was high on Neylan's list as a good person.

I rather suspect that any of the background shenanigans that they might have tried in the university were blocked at the Regents' level. Medical Physics has had some problems; it's now all over and it's now called the Department of Biophysics and Medical Physics, but in about 1960 there were a lot of people on the Berkeley campus who were criticizing Medical Physics as too clinical. There was a cadre of people there, whom I consider rather arrogant, who decide on what's pure science and what's applied science, and applied medical science doesn't belong in Berkeley

--Berkeley's pure. So they were critical of Medical Physics.

As a matter of fact, I did something which I thought was important to do. They used attacks on John Lawrence as a device, and there were just a lot of scuttlebutt attacks. I knew Catherine Hearst, who was one of the Regents. So I contacted Catherine and said John Lawrence's 25th anniversary of being on the Berkeley campus was coming up and I thought he'd done a very important thing in establishing the Medical Physics Division and all the work that went with it. She was very favorably disposed to John; he'd helped the family on some problem. So she arranged on behalf of the Regents to have a 25th anniversary celebration of John Lawrence, at which she asked me to give the address describing John's contribution.

So there have been problems. I think John Francis Neylan must have blocked off antagonism. Then in 1960 Catherine Hearst set up this thing: here the University Regents are feting John Lawrence. How could the people in Berkeley attack this thing the Regents are saying is so great? So it sort of killed a lot of the antagonism then. Those were some of the seamier sides of university things. I generally stayed out of university politics with a vengeance except in a few isolated incidents.

Another illustration of the antagonism that must have existed: I was put up for the associate professorship after two years, in '49, and didn't get it. Then I got it in '50. And then in '54 John Lawrence put me up for a full professorship. He called me in toward the end of the quarter and said, "I don't think it's going through. From the scuttlebutt, we find there are some members of the medical school on your committee and they are very antagonistic." He and I both felt it was really jealousy over the lipoprotein work. But he said they were very antagonistic and the committee recommendation was negative. This was about May, June '54.

Somewhere in July, Professor Birge calls. See, Medical Physics was still part of the department of physics. Professor Birge called me up and said, "Congratulations!" and I said, "What for?" "Well, you've been advanced to full professorship." And I said, "Oh, no, that's a mistake, Professor Birge. I talked to John and I definitely didn't get advanced." He said, "No, you definitely did. The Regents passed it today."

So the Regents overrode this committee on my advancement, which I'm sure was directly [due to] John Francis Neylan, because he just thought I was fine. A little politics went on. So I did get advanced just at that point, as Birge said that day. But John Lawrence didn't even know about it. I went up and told John what Birge said and he said, "Really?" I said yes. He said, "It must have been a Regents' decision."

So, where were we now?

Hughes: Well, you were deciding that you would come to Donner Lab. But why did you?

Gofman Oh, yes. It was A, California, and B, I liked the Radiation Lab. Having worked [there] during the war years, I thought it would be a place well equipped to do research. And I liked Hardin Jones, I'd met [Cornelius] Tobias, thought they were good people. I knew Joe Hamilton from before. He was in the Division, of course. So it looked like a good opportunity in an atmosphere where I had worked, and I thought of Ernest Lawrence as a person who really enjoyed seeing things accomplished.

Early Research at Donner Laboratory

Hughes: Were they hiring you primarily as a physician or as a chemist, or both?

Gofman To do research with this combined background; nothing actually doing medicine per se, but medical research.

I didn't elect to do much with John Lawrence's medical program except the one day a week in his clinic to serve as a physician. I didn't elect to do research with him. I thought I just ought to launch out independently.

Hughes: But you weren't coming with a set idea of what exactly that research was going to be?

Gofman: No, I had no set idea of what that research was going to be when I left the medical school and came to Berkeley. I thought, now, after all this talk about merging a medical education and a chemistry education, to start research... I came into the situation a little differently; I had to think about what I wanted to do. There was nobody suggesting a project to me. I had to generate one.

In those days I had bigger ideas of what was possible than now; maybe it's good and maybe it's not. But as I saw it there was no point in attacking a little problem; you have to attack a big problem. So I said, as I see medicine, the two big problems are cancer and heart disease. Just that simplistic. And so I said, well, let me think about cancer. So I read and I thought and in about three or four months I had no good ideas on cancer research.

I had a little clean-up work to do on that colloid project that I had started. I did that. But I really didn't have an idea on what to do as a major endeavor. I'd gotten interested in the idea of working with large molecules like proteins, so I began to try to set up the lab to study large molecule properties, physical biochemistry. We got an ultracentrifuge, we got electrophoresis equipment, and I had Frank Lindgren join me as a graduate student.

Research on Coronary Heart Disease and Lipoproteins

After the three or four months I couldn't think of an idea on cancer. I said, "I'm just not going to do anything with that. So what's available in heart disease and what ideas do I have?" So I looked at all that was known and unknown about coronary heart disease, which was the big heart disease problem. I wasn't about to go off and study rheumatic or syphilitic or congenital heart disease. Coronary disease was the problem.

I'd read what there was on cholesterol and the arguments about it. There were just these glimmerings about the fact that cholesterol might be transported in some entities that were protein or lipid-protein bonded things. So I decided, let's try to study the binding of cholesterol in fat and blood. We'll start seeing what we can find out about coronary disease this way. So there were two things I thought we ought to do: one is to start studying the production of atherosclerosis in rabbits by feeding [them] cholesterol and studying their blood, and we'd start studying human blood too.

Well, at that time there was a book out by a chemist by the name of Kai Pederson, in Sweden. He was the first associate of the Svedberg who invented and pioneered the ultracentrifuge. It was called The Ultracentrifugation of Serum. So I read the book, and he had in there a curious thing: He said serum is very unfavorable to study in the ultracentrifuge because there's an unstable protein complex in it that seems to dissociate under very strange conditions and it interferes with the analytical diagrams in the ultracentrifuge. So he advised against the study of serum by the ultracentrifuge.

So Frank Lindgren and I got some blood and we decided to see this unstable thing in the ultracentrifuge. Indeed we got the strangest picture on the Schlieren diagram in the ultracentrifuge that you could imagine. It didn't fit any ideas at all. There just shouldn't have been such things. It sort of looked like an artifact. But the one thing one should learn in science is that there is no such thing as an artifact. Everything has an explanation. You may not know it, but it has an explanation. So we fiddled and fiddled with it until indeed if we changed the conditions a little, this anomalous behavior disappeared.

Finally, after some conversations with Ed Pickels, who was the scientist who made the ultracentrifuge--he had come from the Rockefeller Institute to work in this little company that he helped form, called Spinco. Melvin Calvin got their first centrifuge and we got their second one. We showed him these diagrams, and he said, "Well, you've just got to analyze it out by what that tells you. It may require thinking of things that might be moving in a different pattern than what you thought."

The minute he said that I realized what could be going on, namely that we might be dealing with some things that were not sedimenting as proteins would with a force field, but if they were low enough in density they might be doing something different, like floating. That didn't quite explain the whole thing but then we figured out that they might, as a matter of fact, be doing both. In the region the proteins had migrated out of they might be sedimenting, and in the region where the proteins were still present some low-density material might be floating, and it should be accumulating on the protein boundary. And if it did accumulate on the protein boundary, it would give all these bizarre pictures that we were seeing on the diagram.

We had all this reasoned out and said, "Well, if it's doing that, we ought to be able to see it there." By gosh, we looked in one of those cells and you could actually see this band of light-scattering material right on the albumin boundary. Then we ran a preparative run and you could just actually see this accumulation. That turned out to be the lipoproteins of serum.

Then it was obvious what you had to do. The whole idea was you wanted to have it go one way everywhere. So we put in enough salt or sugar--we used both--so that the density everywhere was enough to make the stuff float and then we could see it all the time. Then we made the procedure even better by sedimenting the proteins with enought salt in it, floating the lipoproteins to the top in a preparative centrifuge, and then taking that and running it in the analytical centrifuge. And then a whole world opened up because it wasn't just one lipoprotein, but there was this whole spectrum of all kinds of different things that nobody knew existed.

Hughes: How long did that take, to get to that particular stage?

Gofman: I was there about three or four months fooling around with the cancer thing and not thinking of an idea, so I guess '48, and I think our publications were late '48 or early '49, Frank Lindgren and myself. So I think we worked about seven months.

The story is a little more interesting than that. We wrote two papers for the Journal of Biological Chemistry, one on the discovery of this phenomenon, and an explanation of the bizarre artifacts which were the reason Kai Pederson said you shouldn't study serum. It wasn't that there was this unstable material; it was that if you didn't have the density just right, it could end up anywhere because it was so close to the density of the solution. So we sent these papers in to the Journal of Biological Chemistry and got a letter back from the editor saying, "This explanation is just totally at variance with what people have been thinking. Has the author cleared this with Kai Pederson?"

I wrote back to the editor of the Journal of Biological Chemistry, "No, I haven't cleared this with Kai Pederson, and it is after all a free country and one can think about things that have not been thought about before. I think this does explain the findings and it's consistent and we've got all kinds of evidence here. Just accept the paper or send it back." I got an acceptance from John Edsall, the editor. I could have gotten a rejection, but I think we would have gotten it published elsewhere. But the interesting thing is, within about six weeks I had a lovely letter from Kai Pederson saying, "Of course this is it and how good it is that you found the explanation. It will facilitate the work from here on out." No antagonism at all. So that's how the lipoproteins came into existence.

Well, we already had the rabbits being fed cholesterol. We didn't know how to study their blood in the ultracentrifuge because we got those very peculiar "artifacts" in their blood, as well as in the humans'. But now that we had learned what we had about these molecules being of low density, that you had to add some salt or sugar to bring the solution density above the lipoprotein density, we set the rabbit bloods up and we discovered that they had certain classes of lipoproteins that built up when you fed them cholesterol. They were developing a disease and they weren't showing the usual classes that the rabbits had normally.

This was before I'd even tried to get the coronary series from UC Hospital. I knew somebody at the county hospital in Oakland, and so I called up and said I'd like to get the blood of a patient with coronary disease. I was naive enough to think it might be all or nothing. He said, "Well, we have a lady who's going to be discharged today. If you want to come down and see her, you can. She's about three weeks past a coronary." So I zoomed down to the hospital and there was Sadie Edson. She was already dressed and about to go home and they had told her that I was coming. I told her we were studying coronary disease and we thought we mght find out something that'd be helpful. She said, "Well, I thought all my tests were done." But she finally agreed I could take a blood sample.

I went back to the lab and we prepared it. It was amazing because she had a pattern that looked just like [that of] the rabbits who were developing atherosclerosis. But it turned out not everybody with coronary disease has that blatant pattern. It was a fortuity that she did have it, the first woman I checked.

Hughes: Yes, because you could have dropped it right there.

Gofman: Yes. Oh, gee, we were excited, Frank and I and Harold Eliott, who was working with us. Here was this picture evolving, one thing coming after another very rapidly. It was sort of back to the old days at Gilman Hall. I was working at the lab; we would work till four or five in the morning on the ultracentrifuge just trying to get this data, it was so interesting--after we made that one breakthrough on what had blocked everyone.

As a matter of fact, that was when I contacted the medical school because then we wanted to study a hundred such people, got all this guff, and went to get Tom Lyon. Well, it turned out that everybody doesn't have the blatant thing that Sadie Edson had, but that was the correct set of lipoproteins to look for. We then proved they were strongly associated with coronary heart disease and atherosclerosis in a series of publications. Then we launched that very large-scale project, the so-called cooperative study on lipoproteins and atherosclerosis.

Hughes: Now, is that the one that was involving other labs throughout the country?

Gofman: Yes--Harvard, Pittsburgh, and the Cleveland Clinic.

Hughes: When did that come into being?

Gofman: In late 1950.

Hughes: Oh, all right, so they were already in the picture.

Gofman: They weren't in it actively yet. It was late '50 that I asked for it, '50 or '51 I guess. That was also a little bit thorny. It has an interesting history. What happened was I had a $10,000-a-year grant from the NIH, just on the strength of the early lipoprotein findings, to do further studies on atherosclerosis. When we discovered all this stuff with the coronary disease and the rabbits, I felt we wanted to do a large-scale thing to see if we could predict the people who would get coronary disease.

[Larry Spivak] was editor of the American Mercury at the time and he wanted me to write an article about this heart disease work for the American Mercury. So I said, "I don't really think it's ready to write publicly. If I get to New York, I'll stop in to see you." I applied for $70,000 from the National Heart Institute and they turned me down. They said a $10,000 renewal grant was possible but $70,000 was out of the question for this work. I found out later who was against it; it had nothing to do with UC medical school politics.

But at any rate, this editor of the American Mercury--I went in to see him and explained why I didn't think I should write it. He said, "Well, how's the work going?" I said, "The work is going pretty well, but it's not going to go very much further." And he said, "What do you mean?" And I said, "Well, I just got turned down by the National Heart Institute for a grant to expand the work, to really study a lot of people." He said, "That's incredible." I said, "Well, it just happened." So he picked up the telephone without saying another word and he called up Mrs. Mary Lasker. Do you know her?

Hughes: I know of her.

Gofman: You know her name. Well, Mary's been on either the heart council or the cancer council nationally for years. I heard this conversation going on: He said, "I have this man John Gofman in my office and you have to see him." He was saying, "No, I understand you have a full schedule, Mary, but you have to see him anyway." This went on for a few minutes and finally he said, "Okay, he'll be right over." And he shipped me off to see Mary Lasker in her palatial home on Beekman Place.

She was very interested in heart disease. She said, "Yes, I've heard the publicity about your work. What's the problem?" I said, "The problem's simple, Mrs. Lasker. I want to expand it and see if it really means something with respect to predicting heart disease, in which case we might want to try to do something about lowering these levels of these lipoproteins, and the NIH has just turned me down." She said, "Well, you need help." I said, "What do you mean?" She said, "The only way you can do a big project like this is to have several other places involved. I'll get some people together. Can you stay in town?" I said yes. This was a Thursday. She said, "We'll meet Saturday at my house."

She got Irvine Page from the Cleveland Clinic and T. Duckett Jones from Harvard, a great rheumatologist--all members of the National Heart Institute Advisory Council. We met Saturday and she said, "John thinks he needs some help in setting this project up. It's a very large project." So Irv Page said, "We at the Cleveland Clinic will help," and Duckett Jones said, "Well, Harvard could certainly help." So I got my $70,000 and they each got $70,000 and it was set up as a cooperative venture.

That had its good features and bad. It certainly put us on our toes. We had to work hard. We had to train people from three separate places, and they were always a little bit antagonistic because they were sort of add-ons to a project, and none too cooperative.

Hughes: And they were really just running the bloods....

Gofman: Just running the bloods. That's exactly what they were doing. And we ran bloods; we ran them about two or three times.

Hughes: I know by that stage you had a pretty firm theoretical basis, but with all these bloods coming in to Donner, did you have time to do anything else?

Gofman: I think that is an excellent question, [one] that I've thought about a lot in later years. I did these things: I set Frank Lindgren aside from this massive project and said, "You go work on the chemical structure of the lipoproteins." When Alex joined it, I said, "Occasionally if you'd look in on the big project, fine, but don't get yourself involved in it." So I shielded the key graduate students to keep on their programs on the theory and the whole idea part. Arthur Tamplin came and he worked more directly with me on [the big project], and Oliver De Lalla directly. But Oliver had a program of his own on the high-density lipoproteins.

Hughes: What was his background?

Gofman: He had gotten, I think, a bachelor's in physiology at Berkeley [and in 1959 got a Ph.D. in medical physics]. He did a good job on the high-density lipoproteins. He sort of shepherded in the bloods coming in and served as manager. Plus this girl Beverly Strisower--do you know her?

Hughes: No, I don't know her, but I've seen the name on your papers.

Gofman: Well, Beverly sort of came as an employee and she managed a lot of the operation.

But you've raised a very good question. There were tremendous numbers of exciting things. I finally had about 14 graduate students or fellows at one time, which is way more than anybody should have under ordinary circumstances. In addition to that, I had this massive project on my hands with about 30 employees. I think that it lost a great deal. In other words, I think by my being so much involved in just the management and the fund raising--because the $70,000 didn't cover all the other things, so I was raising money outside--that I really didn't effectively participate as much as I think I could have in the work. Maybe in a lot of ways it would have been better not to do the large project, just let it slowly evolve, and have been able myself to spend a lot more time thinking, working with a few graduate students and not have this large thing. It's awfully hard to say what would have been better.

As it turned out, we spawned one of the biggest research industries in the world now. The lipoprotein research industry is probably, I don't know, $100- to $150-million a year worldwide. All kinds of people are in it and all kinds of people have made major advancements, but I think I could have done a lot better. I did get to do a fair amount of original research, published some papers on hyperlipoproteinemia in families and in various metabolic disorders. Alex and I did some very careful studies of dietary factors that influenced the lipoprotein levels.

I stayed close to the bloods. Every Saturday morning Oliver De Lalla and I went over every single ultracentrifuge film that was processed that week. So of the the thousands, tens of thousands of ultracentrifuge films, I have looked at every one of those personally. We discovered problems in the ultracentrifuge, problems that can arise, and we were able to straighten out the field pretty much. But I regard that as sort of secondary to what I might have accomplished had I stayed with more of the theoretical and metabolic aspects and not tried to move the heart disease field along as fast as I tried.

I can't say anything good in defense of what I did except this: At that stage of my life and with everything breaking just right in the laboratory, [there was] just nothing we could touch that didn't turn out to be something--really a worthwhile insight. It just sort of increased my confidence that all you had to do with a big problem like heart disease is really just barrel ahead and just bust into it. The way you do that is, if you need to study 10,000 people to find out whether it's predictive, you do the 10,000.

But with that tremendous push, even with this big research industry, the advances in heart disease, the number of people we're saving, is relatively few. I have a number of ideas [about] wrong approaches being made in the heart disease field, since I've been out of it actively. But it just isn't that easy to move the big medical problems; there's a lot left to do even after you've made some advances. Maybe I shouldn't have had this idea--if we just push right ahead with it we'll [solve] the practical problem. Maybe I just should have let the practical problem go. But it seemed big; people were dying of coronary disease. "Gee, I'm this far along in it; surely it'll give way before us." So that's why I did it. I can't say that it was a good decision.

Hughes: What made you stop in the end?

Gofman: Well, I didn't really stop. But let me tell you what happened. We finished the cooperative study project. In the middle of the cooperative study project, we learned some additional big insights that you could get from each ultracentrifuge film. It was a big improvement and it was only a matter of reading the films, not doing any of the work over. So we were reading our films by the method agreed to in the big projects and by our own more advanced method.

With the other three labs I said, "I think you should read these films by this advanced method, too, because it gives a lot more information." They refused. They said [they had] only agreed to do the other. Then I said, here's all this work being done and just because you won't read the films, society will be left without this information. So I said, "Can I have your films, blind? I don't need the names of the people. We'll read them for you." They refused that too. I was very disillusioned by that.

So when we finally wrote the paper it was clear the point had been proved, that we could indeed predict the risk of coronary disease related to certain cholesterol and to some of the lipoprotein classes. There was a debate as to how much better the lipoproteins were than the cholesterol or if it was any better, but it was really because we weren't using as much sharpness. It turned out that everything wasn't as neat as Sadie Edson, the first patient. It wasn't as clean a story as that.

Another thing we came to realize, a little late in the thing--it was something we were realizing as we went along--was that the lipoprotein differences you should expect are less the older the age group. We hadn't realized that or we would have studied a much larger group of younger people. We would have had a bigger effect early.

We got into quite an argument in '54 or '55, when we published the results of the cooperative study. Everything since then has swung over [to] where virtually the whole world accepts the work that was done. But there were just a lot of hard feelings about the way that project turned out. My view was the thing to do was to go ahead--Alex and I had done the dietary work--to start pushing the dietary ideas into practice. The National Heart Institute was very critical of me. They said they thought that I was moving too fast, to suggest that people use these dietary ideas. As a matter of fact, one said, "If you persist in this, we'll remove your funding." So they removed my funding in about '56 and then about a month later they restored it on their own. I guess it was just to scare me or something.

Hughes: Was this before the first book?

Gofman: No, that book was out in 1951.

Hughes: So they weren't objecting to that.

Gofman: They didn't object to that. As a matter of fact, some other professors of medicine had put out dietary books with their wives. By the way, the 1951 book has been revised and it's in a new edition. It's being used now and still being published by Doubleday, in its 28th year or something.

So then they gave me the money back and it was '57. There were two things involved, one of which is maybe not a particularly sterling character asset. I looked at the field and said to myself, this doesn't have the excitement it did at the start. There's an awful lot yet to be done. I can think of lots of problems, but does it really need me? With the funds that I did have, the NIH grant, Alex was supported, and Frank Lindgren in some of his work, and some of the others. I thought about it and I decided to take a year's sabbatical, which I did in '57-58. Alex and I wrote a book on dietary treatment and prevention of heart disease, and I wrote a book called Coronary Heart Disease. Have you seen that?

Hughes: No, I haven't.

Gofman: It's a book for physicians. I have one copy of it left, which is this one, my only copy. I will say it's a very good book. What I did is I put together everything about coronary [disease]. Why don't I bring it up?

[pause]

[There is] zero aggression in the publishing business. They do absolutely nothing. They publish a book; if somebody gets their list and orders it, that's all. That doesn't sell books.

Hughes: No, it certainly doesn't.

Gofman: So Coronary Heart Disease did not make the impact I hoped it would, except it educated a few scholars here and there. It's interesting, the methods I developed in the mid-fifties that are reflected in Coronary Heart Disease....

I remember Felix Moore, the statistician for the National Heart Institute, came to see me. I published a short paper that later became part of the methods here, and developed a thing called the atherogenic index, which is a weighted sum of the lipoproteins. He said, "You can't do that." I said, "Why not?" He said, "You're not a statistician. What would you think if I came and ran your ultracentrifuge?" I said, "Well, it's fine for you to run the ultracentrifuge if you want to. I've been thinking about this problem and this is how I would handle it statistically. Of course I'm not a statistician." He said, "You just can't do it." But he couldn't name a reason why, other than the fact that I wasn't a statistician. The method I outlined is now in broad use. But it was illegitimate, not having a union card. Oh, there were many things like that.

Hughes: Well, the criticism that I have read about was concerning the analysis of cholesterol. I guess it was people in the other three labs who maintained that at Berkeley the analysis of the cholesterol was not done as carefully as that of the lipoproteins.

Gofman: No, I don't think that is a correct interpretation.

Hughes: I can't remember where I even saw... Oh, I know where it was. Hardin Jones wrote sort of a rehash of the whole lipoprotein issue in 1970. I'll bring that paper next time.

Gofman: It's not correct, because I can tell you the exact status. Beverly Strisower worked out the cholesterol method we used. There was a lady scientist by the name of Dr. Liese Abell who was working at Columbia University with Forrest Kendall, whom I knew very well, and we did some things together. She had a method. The other labs had been using her method. So we agreed in the project to use both methods. We'd use ours; they'd use theirs.

On a regular basis throughout several years of the project, we exchanged blood samples and it turned out there were slight differences. But the statistician, Felix Moore, was quite happy that if he took all Berkeley values and he subtracted, I forget what it was, 3 milligrams, [they] would be like the others. We had an ongoing quality-control reproducibility test, both on the lipoproteins and on the cholesterol, and there was never any problem about the quality of the cholesterol.

I think Hardin didn't quite understand that right. He wasn't that close to the technical details after the beginning, which is always one of my sad recollections. Hardin Jones was very talented in the lab but, sort of like Seaborg, he didn't want to stay close to the lab. At the very beginning of the lipoprotein project, he wanted to work with us on it and did. But soon after, he got away from the technical details and didn't really understand them. So I think he may have erred in that.

No, the thing that the other labs said really was what I had just mentioned to you before: They said they weren't convinced that analyzing for the lipoproteins gave you more information than analyzing for cholesterol gave you. Therefore, though the project had proved a major milestone in medicine--that people with elevations in these lipoproteins or the cholesterol do indeed have an increased risk of coronary disease--their negative view was, but how much more do we know about the people from analyzing their lipoproteins than we would have known from cholesterol alone?

Hughes: Now, if they had agreed to use your more advanced reading, would that have made a difference?

Gofman: Yes, you see, that's what everybody has learned since then. As the further work has been taken up by people like Don Fredrickson, who is now head of the National Institutes of Health, and Bob Levy, who is head of the National Heart Institute--all the work and the ways we've shown them, everybody now recognizes that you have to study lipoproteins, not cholesterol level.

It's sort of a ridiculous argument that they got into. Cholesterol is just a chemical breakdown of adding up pieces of lipoproteins. You can't get more information from it; you can only get less. In other words, it would be the same thing as saying the works of Shakespeare are still intact after passing a whole volume through a shredding machine. That's what it amounts to. So I was discouraged. With their stating these things, they were just going to block the lipoprotein field for years, these other labs.

So I took off to write the book; Alex and I wrote the diet book. And I wrote a very short book for Putnam's--they published the diet book--called What We Do Know About Heart Attacks, which was a popularized version. And then I figured that after I finished the sabbatical year I was going to give it up and go into some new work. So I asked Alex if he would just like to take over all the lipoprotein work. I was just going to find something new. Then I would participate to some extent in the program. I just felt I'd like to get into something new.

Hughes: When was this, now?

Gofman: '58, '59.

Hughes: I can't find it now, but I did read a letter that [John] Lawrence--I can't remember if he was writing to you or if he was writing to someone else about you--anyway, the gist of it is that you had written to him, and I believe it was in '58-59, saying that you were doing a lot of thinking about your scientific future. Was that what you were talking about?

Gofman: Yes.

Hughes: I mean a complete change of fields at that time. But you weren't thinking ahead toward Lawrence Livermore? That hadn't come into the picture at all?

Research on Trace Elements

Gofman: Not in the picture at all, no. I was just trying to think of what I was going to do. I finally did elect to get into the field of trace elements, partly because of the heart disease work. What had happened was, as I said, I just felt that there was a lot of i-dotting to do. I preferred looking at something totally new, not i-dotting.

There was one exciting finding in Great Britain. A biostatistician by the name of Jerry

Morris, whom I'd met, published a paper showing that there was less coronary disease in regions where there was hard water and more coronary disease with soft water. That was subsequently checked in other parts of the world and shown to be the case. I got intrigued with the idea that there might be something in mineralized water that was a trace element that was involved in the metabolism of lipoproteins. That was the beginning of the era of some interest just in trace metals in general.

So I decided to see whether I could develop a method of studying all the trace elements in one blood sample. The goal was to do as many of the 92 elements as possible. That's where I started after I took that sabbatical and wrote the books and got them out of the way and started working on trace elements. I told John Lawrence I was going to do it; it was fine with him. We got Alex's name on the [heart disease research] grants and finally a few years later my name went off the heart disease grants and they became totally Alex's. For a transition period I was co-investigator on the lipoproteins things.

Hughes: But you weren't pulling other people in the Lab into the trace elements work?

Gofman: No. Alex, Frank Lindgren, Tom Hayes--they all stayed on what they were doing on the lipoproteins, which I think was a very good decision. Those were very viable programs. It's just that I wasn't fabulously excited. I just thought they'd go on without me, is what it amounted to.

Hughes: And they did.

Gofman: And they did, and they did very well. They've done elegant work, and Donner is a leading place in the world in lipoprotein work. As I say, it may be a character flaw, but I can just work like hell in the first five to ten years of something, but then if there isn't some fabulously exciting direction, I want to look at something new.

And so I decided to work on the trace elements. I was trying to see if there was a method that would enable us to do everything in one blood sample. Oliver De Lalla was the only one who joined me in this new work. I looked at the possibility of using neutron activation analysis, and I looked at the possibility of doing x-ray spectrochemical analysis. Neither of these were methods that I initiated; they were existing general methods in the literature. I concluded that neutron activation analysis was excellent but it was only for certain elements. The x-ray spectrochemical analysis would work over a broad range of elements, all the way from things like atomic number 10 or 11 to 92, but the sensitivity wasn't quite good enough to get down in the parts per hundred million, hard to get better than the parts per million. So I thought, well, this field needs some improvement.

So we started to work on x-ray spectrochemical analysis. We developed a way of converting blood samples into a wafer that could be studied. We set up three x-ray spectrometers. We made a hell of a lot of improvements over the literature ability to use x-ray spectrometry. It's a very exacting technique. It's based upon measuring the characteristic x-rays that are excited in an element quantitatively; you measure it by using a crystal to collimate.... Based upon their wavelengths, they come off at different angles from the crystal. Then you count them with the ordinary scintillation counter, just like any other x-ray. So from '59 to '62, I was busy working on that in Donner.

Hughes: Were you also making correlations with specific diseases?

Gofman: Trying to. We thought we wanted to do a number of things. For example, we were going to get on the hard water/soft water problem. We hadn't gotten to that yet. But I was going up to Sonoma State Hospital about once every two weeks and getting blood samples from a number of the children with a variety of retardation disorders, including mongolism and phenylketonuria, to see if any of them were involved in trace element metabolism. Some disorders of neuromuscular function were involved with potassium disorders, which we discovered and never even got around to publishing because the Livermore project came up.

We found a couple of interesting things. One of the things that surprised me has now become quite well known: When we got around to doing the elemental analysis, first in some of the people in the state hospital, we discovered these elevated lead levels--until I looked at our normals, which were running along, and they were also elevated. There's a lot more lead in people than [has been] known, just because of the ambient lead. We picked that up.

And I did a series of about 50 acute schizophrenics in the Napa State Hospital, who were acutely ill with schizophrenia, the first 30 days, and discovered that they had an average bromine level that was very high. I thought, wow, that's exciting. Then I went back and read the early literature and it turns out it's been known for years that people taking excessive bromides, like Bromoseltzer, do in a certain number of cases end up with what looks like an attack of acute schizophrenia. Somebody had years ago done bromine levels on the admissions in a mental hospital acute ward and proved that bromine was elevated. But we just came upon it as a separate thing. And that's what it's due to; it's not a bromine relation with schizophrenia, it's that a certain number of people taking excessive bromides develop acute psychosis.

So work was in good progress. We'd worked out many of the technical bugs. We were moving the sensitivity level down. I gave a paper on it at one of the AEC meetings, had everything arranged in a chart of the elements. They had the mean level of every element or the limit of it. Ed McMillan was just very taken with that presentation. That went on till late '62, when Johnny Foster called me out to Livermore and asked me if I would undertake that thing out there.

Atomic Energy Commission Support

Hughes: Let's not take that step quite yet. The AEC then was funding the trace element work?

Gofman: Yes.

Hughes: But not the lipoprotein?

Gofman: Well, that's not quite correct. They did also fund the lipoprotein work in a variety of direct and indirect ways. They would not fund it specifically as heart disease, but as part of metabolic relationship. As a matter of fact, there were some studies that we had going showing predictions done by [John] Hewitt and Tom Hayes, who's still in the department. They had some work that indicated that lipoprotein changes in animals were very good predictors of whether a certain dose of radiation would or wouldn't be lethal.

Hughes: That was going to be my next question. I was wondering how the AEC looked upon research which was not directly tied in with radiation or radioisotopes.

Gofman: It was forever a problem, Sally, in [Donner] Laboratory. The Laboratory was schizophrenic about it; the AEC was schizophrenic about it. I'm sure everybody in the AEC was concerned that maybe Congress would be critical. I think the AEC in those early days had some what I'd call broad thinkers on what ought to be done, such as Shields Warren, when he was the head of biology and medicine; to a lesser extent, John Bugher. I didn't think too much of him. But Shields had a very broad view and he would, I think, argue strongly that atomic energy can't exclude work on cholesterol because for all you know, cholesterol metabolism may have a lot to do with radiation resistance. That would be his argument. So I think he very successfully defended the breadth of the program in the various laboratories that couldn't be demonstrated to be directly medical therapy with isotopes or use of isotopes as tracers.

But I did have extensive studies going on with Dr. Max Biggs--he was one of the graduate students --on using radioactive tritium in studying the metabolism of cholesterol. And I had Don Rosenthal, who was studying radioactively labeled fat. And then John Simonton and I did a program where we were using radioactively labeled colloids to study one aspect of atherosclerosis, namely whether there were certain cells, called macrophages, that were migrating into the arteries from the liver. We were using radioactively labeled cells. But nevertheless it was a problem. As I said, they were all schizophrenic about it.

Probably I was the major outsider, so to speak. Tobias's work was mainline AEC work. Hardin Jones was a borderline case. A lot of [my work], you might say, is that really AEC work? But I will say this, that John and Ernest Lawrence were quite happy about a lot of the AEC funding going into my work, and it did. John worried about it, maybe it would be criticized more, but Shields Warren and Chuck Dunham, who replaced Bugher--Dunham had been Shields Warren's right hand--they were supportive. And I would say, probably, Sally, over the years before I did the trace element work, which was clearly the kind of thing AEC could support because it was using x-ray spectrometry--the years from '47 to '60 on the lipoprotein work, probably half of the money I spent was AEC money. So I was well supported, in addition to the heart disease funds from the NIH and some other grants I had.

Hughes: That made it easier for you, didn't it? Because the AEC money came in through John Lawrence, so you didn't have to stomp around....

Gofman: It was worlds easier, because to raise money--as I did have to, in addition to what the AEC provided --raising a $10,000 grant from a foundation or company is a job. [It] needs a lot of work, a lot of paperwork. For the AEC, I'd write my little section of the annual submission; the money came in as a package and you had your share of it. I was very generously supported. I had those little bumps in trying to get the heart disease money that Mary Lasker had to help me with, but overall during my entire period at Donner, I couldn't have asked for better suport financially.

With what I'd say is a limited amount of my time going into fund raising, probably my problem that I didn't get to do as good work as I might have was more taking on this big managerial job, not the fund raising per se. I couldn't say the time I had to take for fund raising wsa what interfered with my [research] time. It was really the manager jobs of this big project. I was very, very generously supported, couldn't have asked for better. I had everything I wanted. Anything I didn't accomplish was my fault.

Hughes: How were you regarded by the rest of the Lab, do you think? It seems to me in talking to you and to a certain extent to Nichols and to Hayes--they haven't said so overtly, but the impression I get is of almost a unit within a larger unit, a self-contained unit, more so than John Lawrence's group or Hardin Jones's group or Tobias's group. Some of that, I believe, is just the nature of the research itself, which was--

Gofman: Different.

Hughes: More different than the rest of the groups were different. But is there more to it than that? You had a lot of money flowing in, you had a much larger group of graduate students than anybody else....

Gofman: Yes, at one time.

Hughes: Was that a source of jealousy?

Gofman: I think there was some jealousy, yes. It wasn't really very overt nor ever came to anything that I could say was even unpleasant words; it would have to have been beneath the surface. I think there was some jealousy. This isolation was for real, as you say, in part because our work was a wholly different area, away from others. We got a lot more national and international publicity. I think that wasn't helpful from the point of view of others in the Lab, but we put Donner Lab on the map as a scientific institution--I mean as a credible scientific institution.

I wasn't meaning to in any way denigrate John Lawrence's work or Tobias's work, but the medical and scientific community learned about Donner Lab through the lipoprotein work. And especially when you consider that the now director of the National Institutes of Health, Don Fredrickson, and the director of the Heart Institute, Bob Levy, both concede that their work in heart disease was an offshoot of the Donner Laboratory work--they're very, very generous in their writings about the fabulous contributions of the Donner Laboratory group. So we did have some impact in putting the Lab on the map in that sense.

Not in the atomic energy circles. In atomic energy circles we were a very definitely non-mainstream effort. I mean, if you'd ask at the Brookhaven Lab or at Rochester, they'd know about Tobias's work or they'd know about John Lawrence's; they'd know very little about ours. Those are atomic energy labs. But we were well tolerated.

In a lot of ways, I'm critical of myself. As I say, when I get to working on something I tend to work pretty hard. So during those lipoprotein years, from '49 to '56 or so, I was working in the lab many, many long hours, late at night. A lot of the graduate students, some of the people would work with me late at night--12, 1 o'clock in the morning Donner Lab was aglow. But it was only our section of the building. And I think others resented that; that they were sort of outsiders.

There was a certain amount of clannishness, or whatever you want to call it, associated with the work. I enjoyed working late and did, so I was there a lot. But oftentimes I remember at midnight Arthur Tamplin, John Hewitt, Alex, others would go down to get some food and refreshment, then go back to work at the Lab. There was a difference between our group and the rest of the Lab, but certainly nobody ever made any difficulty.

Hardin Jones

Hardin Jones was extremely supportive at all times. I wish he would have been less supportive and more involved in the work. Hardin Jones was an important figure in helping that work because he was always a spokesman on our behalf in anything that involved the rest of the Lab.

Hughes: Tell me a little bit about him, since there's no direct way of getting to him. He became associated with the lipoprotein work right at the very beginning, did he not? How did that tie in with his theory of aging?

How are you doing timewise?

Gofman: We're all right. This guy is not supposed to be here until [later]. He's a guy from the Attorney General's office of the State of Nevada, wants to ask me some questions. He wanted to know if I'd testify and I said no. I just can't afford the time. I get a little bit irritated that some of the things I've put out in publications... Why should I go testify before the Nevada State Board of Health when they can read it?

How did it fit in with Hardin's theory of aging?

Hughes: I'm not clear even when he really began....

Gofman: The aging theory work, I'd say, was about 1953. Hardin took a year off and he and Helen went to Sweden on sabbatical and that's when he did a lot of his work on this theory of aging and on his concern that there was a big error in the claims about cancer therapy.

Now, he had already helped on the lipoprotein thing from '50 or so on. He just sort of came into the work; it wasn't by any prescribed thing. We were doing a lot of interesting little experiments, experiments on ourselves--like we'd do some dietary studies on ourselves that might involve drawing bloods for a period of 48 hours or 96 hours. Hardin would participate and help set it up, and sort of got into the work that way. But the ever-present difficulty was Hardin was always damned willing to be supportive but never aggressively scientifically active in the lipoprotein work. I wish he had been, because it would have been a tremendous assist if his brain was focused on that problem. It would have been a big help.

Hughes: Isn't that true pretty much across the board? It wasn't really just the lipoprotein work, because the pattern of his research seems to me to get further and further away from the laboratory as time goes on.

Gofman: Yes, it did.

Hughes: Until at the end, why he was in Donner Lab at all I don't know.

Gofman: It happened to Hardin. He incidentally was extremely gifted in the laboratory. I observed him in '46 when I came over during the time of that clerkship. At that time Hardin was working in the lab. He was very good in the laboratory, just a good experimenter.

Hughes: How do you explain that?

Gofman: I don't know. He wasn't in the Seaborgian sense an entrepreneur. In fact, I don't think Hardin ever did really push himself as a figure, but he just liked to be involved in supporting everything rather than doing. That year in Sweden I think he was more productive than he was thereafter for a long time; just didn't seem to settle down to any specific thing in the lab. But I oftentimes suggested on the lipoprotein thing, well, why don't you take this area over and work with the guys on it? He'd say yes, but then he wouldn't do it.

Hughes: And ironically enough, he all this time was assistant director in charge of the scientific aspect of the Laboratory. What do you have to say about that?

Gofman: He was aware of the scientific aspects of what we were doing, but not deeply and intimately. He was aware of the scientific aspects of what a number of the others were doing. So he really sort of knew what the impact and value of the various projects were, but he wasn't in any one of them.

Hughes: What use was that knowledge, though? How was that helping the Laboratory?

Gofman: His knowledge, you mean?

Hughes: His knowledge. I mean, as an assistant director, what was he really accomplishing in that position?

Gofman: Well, there were accomplishments where you had to at times talk with AEC on the outside.

Hughes: And he did that?

Gofman: He would do some of that translation, yes. He could do some of that. He occasionally would go to the biomed directors meetings. John Lawrence hated the biomed directors meetings, which were every three months. They were at some lab, either Donner or Brookhaven or Hanford or Oak Ridge. John Lawrence hated to go to them, but he'd put in an appearance once in a while, because even in those areas I think people sort of regarded John Lawrence as someone aloof from other people. I think they sort of resented him; he didn't seem like one of the guys.

Hughes: And he felt that. Is that why he didn't go?

Gofman: I don't know how it was, whether it was his shyness. But he'd come down and ask would I go this time, or would Hardin. Hardin went to at least half of them, I think. So Hardin did do some of the AEC thing. And I think there were things at the university level where Hardin was defending the work of the Division in university circles against some of the attacks on John and the Division.

It is regrettable that he wasn't more involved in the scientific aspects of the Lab later. It was not through any lack of ability. He certainly had it. That work he did in Sweden, the year on aging and the cancer thing, I think that cancer thing was right. He shook up the cancer therapy community pretty badly. They criticized the hell out of him, but they never showed he was wrong. I think he's still right on that today.

Hughes: Did that ring any bells for you at that stage, or was it too early?

Gofman: On cancer?

Hughes: Yes.

Gofman: No, I just looked at it; it looked interesting. I wasn't even thinking about that. No, I just didn't give it all the attention it deserved.

Hughes: How was his theory of aging received by the scientific community?

Gofman: I think it got pretty good reception in all quarters.

Hughes: So he was regarded as a credible scientist by outsiders?

Gofman: Yes, I think by outsiders Hardin Jones was regarded as credible. I've even seen recent allusions to his work, in a credible way--his early work. Hardin Jones was regarded as less credible in his later years, but I think that early work was regarded as good.

Hughes: What was his relationship with John Lawrence?

Gofman: It is my impression that certainly during a large number of years that I knew them, let's say up to '55 or '60, John Lawrence was very closely related to Hardin, relied on Hardin quite extensively to do things. And then Hardin really carried the ball for John on administrative matters quite actively. He definitely had John's ear more than anyone else.

Hughes: Scientific adminstrative matters? Remember, Born is in the picture too, at least from 1950 onward.

Gofman: Was Born as early as '50? I would say Hardin would be more who John would rely on scientifically, yes, at that point.

Hughes: Yes, Born was doing the straight adminstration.

Gofman: And helping with his medical program. Born was doing adminstrative work and helping with the pituitary program and so forth. Born served in his capacity as a physician with John Lawrence. But I think Hardin had a closer association with John during that period. In the period from about '55 to '60, I don't know really why, John Lawrence came to lean on me a lot. Always wanted to have me come up and talk to him about certain things. Wasn't '57 the year that Ernest died?

Hughes: Yes.

The Death of Ernest Lawrence

Gofman: Do you know the circumstances of that death?

Hughes: I'd like to hear it again. I've read the biography by Childs on Ernest Lawrence. Have you read that?

Gofman: I haven't read that, no.

Hughes: It's not a very detailed account.

Gofman: Well, Ernest had this ulcerative colitis and he had ups and downs and finally they just thought that surgery was the thing to do. John had recommended the surgery. They were very close and [Ernest] relied on John medically. John talked to all the medical people. He finally said he thought he'd recommend surgery.

So Ernest had this surgery, and his blood pressure didn't come back up after the operation; in fact it kept going down. In about four to six hours, he went into circulatory collapse and died of circulatory shock. Everything had gone all right in the surgery; there'd been no untoward things, but he just didn't have a recovery. So an autopsy was decided on and we went.

John was really, really broken up. It turned out that--strange quirks of a small world--the ulcerative colitis did not kill Ernest; atherosclerosis killed him. And here's how it was: There's an artery that supplies the upper part of the abdomen, the intestines, other organs, the upper parts. It's called the superior mesenteric artery. In Ernest Lawrence, that artery was blocked by an atherosclerotic plaque, closed off. Nobody knew it.

In life, when this happens, often the way you stay alive, and maybe don't even know you've got anything like this, is that new channels develop from other arteries, so-called collateral channels, a subject we've studied some in our work. And so Ernest had collateral channels from the lower part of his intestine supplying the upper part, and when that was cut out, you inadvertently cut out the source of his blood supply for his whole upper abdomen. But the superior mesenteric artery block was not found in surgery; it was found in autopsy. Nobody knew it existed and nobody knew that by the operation itself they had taken away the blood supply. We found this out in autopsy.

Hughes: I had not heard that.

Gofman: Well, it's just a shattering story. And it's sort of ironic, because here we had been studying atherosclerosis, and this very thing, a complication of this disease, happens to our director. John just went into a tailspin after that, more than just the brother thing. He just felt that he had done the wrong thing. I spent about two years trying to get John Lawrence back on an even keel.

Hughes: How could he have felt responsible for that?

Gofman: He advised the surgery. Of course it wasn't rational but you don't know John. On some of these things, it isn't rational. He'd come down to my office or he'd call me up there and he'd want to go over this thing, months and months and months afterward. We got to be quite close during that period, as a matter of fact. As I say, that was a period when he would sort of rely on me for advice on lots of things in the Lab. But I think Hardin still was very high on his [list]. He regarded Hardin highly up through that period at least.

The Donner Laboratory Directorate

Hughes: One of the early people who isn't mentioned is Tobias. He doesn't seem to be an intimate of Lawrence's particularly.

Gofman: I really think he is, though. John would rely on Hardin more on broad matters of the Laboratory, because I think he felt that Hardin or I might have a broader view than just limited AEC radiation biology work. I think he had a high regard for Tobias's work and for Tobias, but I think he also did not have too much respect for Tobias's judgment in a broader sense. Toby had I think a little bit of a reputation of--he was forever getting himself over his head in money, committing that he would do things and then having to be bailed out economically. So I think, at least in those days--maybe it's very different now --Tobias was sort of a problem child with respect to spending money out of the budget.

But I believe that John regarded that the hard-core radiobiology of Donner Lab was being done by Tobias and his group, that it was good work, and that he respected it highly. I don't think he called on Tobias to be more of a broad figure in where Donner ought to be going.

I think I worried John some on the grounds that I might be more breadth than was healthy from the AEC point of view. He worried that they might feel that too much money was going to this thing that was borderline.

I think he thought highly of Tobias's work but didn't think Tobias would be a good manager of the Laboratory in general. That's my appraisal of it. But I don't think at any time that he had less than a very high opinion of Tobias's ability and the field he was working in. He was really the main-line radiation biology work at the Lab.

Hughes: What about John Lawrence as a director?

Gofman: Well, I've heard a lot of people critical of John Lawrence in many ways. It was sort of like, after Ernest's death, he could just go over something and over and over it; we would talk it through and then a month later, three weeks later, we would go over it again. Many people felt that maybe John Lawrence never made up his mind on anything and they were critical of him, but I had a rather good opportunity to view the man in operation. I think John's not pleased about the positions I've taken on things--on radiation hazards in 1969 and 1970 and the whole ruckus that came out of that. But observing John Lawrence from '47 to '62, when I was there full-time, then went out to Livermore and came back in '73--but I was no longer working in the Lab, I was teaching only--I think John Lawrence was a good director of that Laboratory and I'll say why.

As I had an opportunity to think about it and look at it, even though he may be unhappy about some of the positions I've taken recently, I regard him as a good director. Some might say a director of a lab ought to have the laboratory more in his grip. Maybe that's good and maybe that's terrible. In my view, John Lawrence had a number of young, aggressive scientists there when I came--for example, Tobias--who had ideas and wanted to work. And he showed a remarkable ability to support us and not interfere with us.

I can't remember a single time when John said no, you can't do this, or criticized me for doing something. So he was very supportive and let you do your thing. I think that's a good way to run a lab if you have people who are excited about what they're doing and are going to do it. You've got to have somebody provide leadership in a laboratory for groups or the main leader. He didn't try to provide much direction to the individual groups, but he let us alone.

Hughes: But then under him you have Hardin Jones, who also is running a rather loose ship. The total impression, and I'm not meaning that this necessarily is bad, is there isn't much direction. There's nobody that is really in charge.

Gofman: That's correct. See, I think that's exactly correct. So I was in charge of my program and I knew pretty well what everybody was doing in my program, although I think I could have done a better job of time with the individual scientists, like Alex or Frank Lindgren or Hayes, had I not had that big cooperative lipoprotein study to manage. But my ship was pretty well under control, and separately Tobias's area was very much what Tobias wanted. So that if you pick out individual parts of the Laboratory program...

Then later, when Nichols and Lindgren were off on their own, their stuff was tightly controlled by Alex and Frank. They knew what they were doing. So I don't think it really needed too much direction.

Hughes: But then there comes a stage--and now I'm talking about after your time as a researcher at Donner

Lab--when things do begin to fall apart, or at least that's my impression. I think it's pretty well documented that Donner Lab does get into a rough period in the late 60s and 70s, when perhaps a firm director--

Gofman: Might have made a difference. Yes. I think that's quite possibly true. I sort of lost direct touch when I was at Livermore, from '63 to '73. Up through '63, when I left Donner, everything seemed okay. But, see, I was managing my show, Tobias his....

Hughes: Some of it's external. The funding, of course, becomes more and more of a problem across the nation. So there's stress from that angle.

Gofman: Right.

Hughes: And also you have the question of age. I am certainly not competent to judge that one, but there's no question that by 1970, when Dr. Lawrence stepped down, he was old to be a director of a laboratory. Was that a factor as well? Was the whole institution changing?

Gofman: Probably. I think maybe the right steps were not necessarily taken on getting a younger group moving up to guide the Laboratory. He's been heavily criticized, but as I say, during the period I was there as a researcher, I found it was good, that he was just about right. He was supportive, helpful, never seemed to evidence real jealousy about accomplishments of the younger men. I didn't think one really needed the director to be a scientifically active person. But maybe there weren't enough people watching over fairly big segments of the Laboratory. Say, in 1970, maybe it did sort of fall apart. I really wasn't watching it closely.

Hardin was sort of off in left field by then. I remember once in about '69 or '70 having a talk with him. Oh, Hardin just didn't make a lot of sense to me in some of his activities in that whole drug field. It may be good work; I don't know. Do you know? Have you got any feedback on that?

Hughes: Well, the impression that I've gotten from talking to a few people about that particular aspect is that it certainly did not help the Lab, just by the fact that it was controversial. Of course it wasn't just the drug business; he was tied in with the Free Speech Movement and all that. The very fact that he was spending so much time outside the Lab could not have helped in any way.

Gofman: Well, he wasn't being a scientific figure in the Lab. I don't really think in those years that Hardin was keeping up with what was going on in science per se. I have a lot of questions in my mind. I left the Lab maybe at the wrong time. I might have done some good by staying.

The Livermore venture had some value. Because they really didn't want to stand behind honor and truth in science, I found the Livermore Lab directorate just shocking to me, as everything that Ernest Lawrence wasn't. It was, in my view, the opposite of what Ernest would have wanted done there. I have had and still have very strong feelings of ties to the name "Lawrence Laboratory." I was proud of it, proud to be a member of it, and I feel rather badly about what happened--the whole denouement of the Livermore situation was just so opposite to what I considered the action of a great laboratory. And yet, in some ways, maybe if I hadn't gone out there I could have helped more at Donner in that transition period; but you can't know it.

Hughes: Yes, that's retrospect again. Well, I don't want to get into the radiation question now. That is a big question. So maybe this would be a good point to stop for the day.

Lawrence Livermore Laboratory, 1954-1957

Hughes: Let's take you back to 1963, when you were moving over to Lawrence Livermore. First of all, can you tell me what the pros and cons of that rather drastic change in your scientific career were?

Gofman: To go back a little bit before that, how I even got to know the Livermore people: In late '53, Ernest Lawrence called me into his office and said, "I'd like to ask you to do me a personal favor." I said, "What's that?" He said, "A lot of our good scientists have gone out to Livermore and I've gone out there and I think they're doing things in a way that may be harmful to them." One thing about Ernest Lawrence, he was always tough on expecting people to observe safety rules in the laboratory. He was also tough on pushing things through, but he did want people to work with safe conditions.

Hughes: You're speaking now of radiation.

Gofman: Yes, radiation safety. He said, "You're the only one with a scientific training and a medical degree that we have. I wonder if you'd go out there a day or two a week and just go looking around. If there's anything you're unhappy about out there you can tell them to shape up and that your statement carries my personal word." So I said, "Sure, I'll do it."

So starting about then, which is either late in '53 or the beginning of '54, I went out and saw Herbert York, who was then the director of the Livermore Lab. Ernest had already briefed him that I would be coming out. So I decided to stay two days a week. Since I didn't have a base out there, something to fill in when I wasn't wandering around, I accepted the responsibility as the head of the medical department of the Livermore Lab.

So from 1954 to 1957 I spent two days a week at the Livermore Lab as the head of the medical department. I didn't stay in the medical department; I actually roamed around and watched various operations, preparations for Pacific [atomic bomb] tests. I raised hell with a number of the chemists, especially, who were doing things very unsafely, such as putting their film badges in a drawer in a different building when they were going to do a dirty job so the film badge wouldn't show the exposure.

During that period, some of the chemists and such physicists as John Foster, who was running one of the weapons divisions, had some questions about some of the potentials in the Pacific tests. So I did a number of calculations for them on the potential biological hazards, and through that got to know them.

I think it was 1955 or 1956 when Dr. Max Biggs, who had gotten his Ph.D. with me and gone off to Indiana--he was also an M.D.--wasn't happy in Indiana. So I told him to come back and be in the medical department at Livermore, that he could do research there and that I was not going to stay there very long, and that he'd be the medical director. He is today. In 1957 I decided that the place was stabilized enough. Max could do the job that Ernest had asked me to do and so I left and came back into Berkeley full-time and Max became head of the medical department.

The Biomedical Division

John Foster became director [of Lawrence Livermore Laboratory] when Harold Brown went to the Pentagon. Johnny knew me from my having helped him during that '54 to '57 period. He called me in in late '62 and said, "We have an approach from the AEC to set up a biomedical division here and we want your advice on A, should we do it, and B, would you consider doing it?"

Roger Batzel, who's now the director of the Lab, was associate director for chemistry. He was very close to Johnny so he was in on all those conversations. I said, "Well, I don't understand this, Johnny. The AEC is not in the habit of just giving out plums. What's really up?" He said, "What's really up is that Glenn Seaborg and the other commissioners have been getting a hell of a lot of flack from the fact that in the '61 and '62 tests in Nevada we clobbered the State of Utah with radioiodine."

The reason that occurred was, in the earlier tests the network for measuring radioiodine in milk was not really in place. But by '62 it was, and so here by setting up this milk network they had hoisted themselves on their own petard and the people in Utah were furious. There were all kinds of protests to the Atomic Energy Commission. He said, "What Seaborg and the commissioners think is that somehow if there were a biomed division here helping us plan every test we do and supervising the evaluations, we wouldn't get into this trouble." I said, "Well, that is not very sure, in view of the fact that you can't very well be having atmospheric tests and not be dumping stuff on people."

Hughes: Were you at that stage aware of the hazard?

Gofman: I was very vague and taking an erroneous position myself at that stage. Prior to that, in 1957, Linus Pauling had raised grave questions about the hazards from the weapons testing and estimated how many people would be killed at low doses from it. I am on record as having made the utterly stupid statement that yes, Pauling's calculations would be correct if the effect held down to low doses, but we don't know whether it does or not.

I didn't say there was a threshold, but I said we don't know. That is when I took my cardinal, the most magnificently stupid position of my life, when I said [if] you don't know, you just go ahead and you don't interfere with progress. That's a statement I've later referred to as the equivalent of the Nazi doctors, a willingness to experiment on human beings.

I feel very badly about that 1957 position, but not nearly so badly as I feel about the scientists in 1980 who are saying the same thing. They seem to have learned nothing in 23 years. I at least did very shortly after that. But at the time John Foster asked me, I had not yet [retreated] from the position of saying, "If we don't know, we can't stand in the way of progress." But I certainly was not saying that we knew the answer.

Hughes: Look at the difference in your environment then and the environment now. Linus Pauling was saying that, but who else was?

Gofman: That's right. So Johnny said, "What do you think of this?" I said, "Well, let me just tell you very frankly what I think before we go any further. I don't really think the AEC wants to know the truth about radiation hazard." And John Foster said, "Neither do I." I said, "Second, I do think it's important to find out the truth." He said, "What do you think of the idea?"

Hughes: Was the Pauling data in the back of your mind when you said that?

Gofman: You mean that the AEC didn't want to know the truth?

Hughes: Yes.

Gofman: They tried to crucify Linus Pauling through the Atomic Energy Commission, and tried everything in the book to castigate him personally and what he said. It was clear the AEC was not endeavoring to find out the truth. I was using AEC money, you can see that in my lab, but not on this subject. But I certainly didn't have a high regard for how they were handling the situation in the whole Pauling-Teller debate on radiation hazard and weapons testing hazard. They were simply not being very candid.

So I said, "I really don't think that the AEC wants to know the truth. But I do think, Johnny, in answer to your question, that the truth is very important for society to determine." These are late-'62 discussions, maybe January of '63. So I said, "And because I don't think the AEC wants to know the truth, I would not consider taking this job out here, though I think the subject's important."

So he said, "Let me tell you something." I don't think Johnny was really quite candid himself when he said, "I'm going to make my life in this laboratory because I think it's one of the most important laboratories in the world. I have no desire to go to Washington or anything like that." He later did go at the first chance he had. He said, "You've been a member of the Lawrence Lab for a long time." I said, "Yes, and I think very highly of it too, and I have been out for three years helping with the Livermore branch of the Lab at Ernest's request." And I did have a very great sense of pride in the Laboratory overall.

He said, "Well, look, do you think for a minute that I personally or the guys who are associate directors of the Lab would tolerate the AEC interfering with the establishment of truth if they set up a biomedical division?" I said, "I would certainly hope that would be the case for the people at the Lawrence Lab." That's all I said, I hoped it would be. He said, "Let me tell you, they'd stand behind you on anything you did, no matter what. [Do you think the Regents of the] University of California would tolerate the AEC interfering with scientific freedom?" I said, "I don't really know, Johnny. I would hope they wouldn't, but I don't know."

I will say one thing for the Regents in retrospect: In the entire controversy that I got involved in with the AEC, the Regents at no time ever did anything that I would consider unfavorable to me. And when I came back to Berkeley, my position was fully restored. So I certainly have total respect for the Regents' actions in the whole thing.

At any rate, these were Johnny's reassurances, and I said, "Well, they're reassurances, but I really don't think that's enough to make me consider it. Let me just say this. I'm happy in Berkeley. I've finally gotten rid of my big project that I had, with 30 or 40 people working for me on this massive heart project in addition to all the graduate students. I now have just a few graduate students; I'm back in the laboratory working; I have a total of only six or seven people associated with me and things are just going the way I want them to on my x-ray spectroscopy work, and I like the teaching. Why in the world would I want to be the head of a biomedical division and an associate director of Livermore? I'd be pushing paper instead of doing scientific work and I've now gotten rid of most of my paper-pushing and I'm really doing scientific work and enjoying it."

He said, "Hey, wait a minute. You could do even better at Livermore. Look at the support we have. We've got engineering, mechanical, electronic [facilities] that could be put at your disposal. We have the best computer facility in the world. We have and will build you more space. If you just spend a year organizing the division, you can then have just everything you want to work in the lab and you can just give up all this administration, but we'd need you for a year to organize."

So this went on for about two more months, discussions back and forth; I talked in detail about it with Alex Nichols and he was possibly going to go with me. Mostly my concern was that the Atomic Energy Commissioners were not to be relied on in their sincerity. They were in trouble because of the Utah flack. I really didn't think too much of the idea that our supervising the weaponeers could do very much with respect to reducing fallout. I really suspected that a large part of the thing was that they mostly wanted it for the publicity announcement that they were setting this up.

So I said, "I'll help you prepare the proposal, but I won't say anything on whether I'll do it." I talked with John Lawrence about it; he was opposed to it.

Hughes: Why was that?

Gofman: He just thought it was unnecessary and it might hurt Donner. He thought that while the AEC would say they were going to provide the additional funds for the Livermore Lab, in time they would regard Donner plus Livermore as still University of California and that both labs would get less money. I think in part he proved correct, and part wrong. They tried that, but they didn't succeed too well. They tried to squeeze on both by saying it was one lab.

I went back and forth on it. By then Johnny was pressing harder. He'd already gotten Clark Kerr and the Regents to sign a letter stating that I could cut my teaching down to 10 percent, that I would not be giving up my professorship--I would be on leave from full-time teaching--and I got it in writing that at any time that I was for whatever reason unhappy about the Livermore arrangement, I could come back into Berkeley and resume my full-time professorship without any questions. That was how unsure I was about AEC. I got that in writing before I even said whether I'd consider it or not. But he got that from Clark and the Regents. Finally I decided this might have some interesting possibilities; I'll do it.

You asked me what made me make this large decision. It's awfully hard to say. I really think in some ways one would have to say that there must have been some lapse of my good judgment and cerebration to make that decision, because I surely did not need that headache. I really was happy in Berkeley, working very effectively. I had gotten my x-ray spectroscopy study of the chemical elements going well by then; that was late '62. I'd started on that in '59, after I did the books. And so I felt that I was gettng along all right, but I did have this lapse and decided there was something important about doing this.

I went back with Johnny Foster for a last discussion with the AEC people about this, before we decided to formalize it. Charles Dunham, who was then director of biology and medicine of AEC, was there in the room, Leland Hayworth, one of the commissioners, Glenn Seaborg, and sundry other officials of AEC. I'm not sure but what Robert Underhill wasn't there; he may have been. One of these people from the Berkeley accounting department was. It may have been Underhill. And John Foster and myself.

In the course of that discussion, I came out and said to Glenn, "I really think you ought to think twice about putting me in charge of this Livermore program. I'm not talking about the program itself, but you ought to think twice about putting me in charge." I said right in that room, with all of them assembled, "Frankly, if I take this position, I really don't give a damn about the

AEC's programs. We're going to work on the biological and health effects," because the whole mission was "Implications of All Nuclear Energy Programs upon Man in the Biosphere." That was the title of the project. I said, "If our results are such that they hurt AEC programs, we're going to publish them and say so, what the results are. So I think you ought to think twice about it."

Glenn Seaborg quickly came back with a statement: "Well, Jack, all we want is the truth." What the Atomic Energy Commission really wanted was never to have the truth--I state that flatly and unequivocally. I think that was the most insincere organization I have ever seen, though there are many insincere organizations.

Hughes: Well, how do you explain the fact then that they did go ahead and hire you?

Gofman: That is interesting. I think they felt they could always control it. It's really complex. They probably figured people around me, like Johnny Foster, might very well remind me of what is appropriate behavior and so forth. I don't think it was smart of them to give me that job. I made it clear.

Hughes: All they had to do was look back a few years to the lipoprotein controversy to see that you were a man who held to your point. If they wanted somebody to be a patsy, so to speak, you were a very poor choice.

Gofman: I think they made a mistake in choosing me. But that's what happened in that room. Glenn said, "All we want, Jack, is the truth."

Hughes: Do you think it had any bearing at all, the fact that you were a graduate student of his?

Gofman: Yes. I think he had a high regard for my ability and work. I had done a good job at Berkeley when I was working with him in the war years. So I think that had some bearing. We were friendly all during the period from '47 to '60, when he went off to head the Atomic Energy Commission, but not terribly close at all.

Chromosomes and Cancer

Gofman: My mainline effort in the Laboratory was on my program in chromosomes and cancer, where we were testing the concept that a specific chromosomal imbalance might be the cause of cancer.

Hughes: Did that get dropped when you really got embroiled in the radiation-effect question?

Gofman: No, it got dropped in 1972 for another reason. I'll tell you in just a second, or any time you want to know.

But this was an idea that was tested in the thesis of a German embryologist by the name of Theodore Boveri. I think it was a very important concept. We made real quantitative progress with that in that research. One of the few sad things about the recent era in my life is that I really feel that it was a grave loss to me personally and to society to lose that program on chromosomes and cancer, because I think it was important work.

Hughes: Had you gone to Livermore with the chromosome problem in mind?

Gofman: No, that was one I had decided on there. I was still working on trace elements when I went to Livermore. But getting in the administrative thing, I just handed that over to Lynn Anspaugh, who was working with me, and the chromosome thing came up later.

Hughes: And was that through reading Boveri's book?

Gofman: Boveri's book, yes, that's right.

Hughes: I believe at one stage you were trying to work with fresh material, cancerous material, and were unable to. Can you explain that?

Gofman: Yes. Let's see, we were using a method where, like everyone else does, we spread the chromosomes out and then we made tracings of them and then those tracings were analyzed by scanner computer. We have three publications on that work in the literature. But in order to do this thing and make the tracing, you needed to have material spread out very well. When we did the work with the normals, we were just having no problem at all.

Then we started to study what are called cell lines. These are cancers grown in test tubes; they're called cancer in glass. They are from the American Type Culture Collection in Washington, kept frozen. They can unfreeze them and people could always have the start of a new culture. These things never die out, in contrast to normal cells.

One of them is known as the Hela cell. There are 18 of them that we had at that time. You can keep growing them indefinitely. Normal cells, when you subculture them, after 50 cultures they die out. There's a man by the name of Dr. Leonard Hayflick who has an idea that that's part of senescence and that the characteristic difference with cancer [cells] is that they never die out in culture.

Well, we started working with these cell lines because they were easy to work with. They were cancer in glass, and we could make beautiful preparations. We did the first part of the research with these cell lines versus normals. It all worked out very well. We discovered this one consistent relationship of a chromosome that has the dimensions of what we call the E16 chromosome--but, as we pointed out in our paper, since we're only measuring dimensions, it could be a piece of another chromosome with these properties. But we had a very powerful statistical association. Then we thought, but these cancers that grow in glass are special. They're the ones that people succeeded in growing in glass. We could hardly say that's representative.

So I started contacting friends at various hospitals in the Bay Area to see if we could get, first, what are called malignant effusions. In certain cancers, certain stages, they accumulate fluid in either the chest cavity or the peritoneal cavity. There are often cancer cells in these fluids; that others had shown. So we started to get some of these. They were a very good extension of the studies because we could generally get good preparations. But occasionally we would get very poor spreading of the chromosomes out on the slide from these effusions. But more of the effusions worked than didn't work, and we added that to our list.

Then I contacted a lady who was the chief surgeon at the V.A. hospital in Livermore and said we'd like to start getting fresh tissue right from the cancer, solid cancers at surgery. And we got about 11--no, 11 of them we got to study; we got about 25 of them. Most of them, we simply couldn't make a good preparation. They wouldn't grow in culture. My biopsy people, myself and others, grow cells fine from normals. We couldn't culture the cancer cells from these fresh cancers. We tried making a fresh preparation without culture and the chromosomes just wouldn't spread out; so technically we were bugged at that stage by not being able to make a decent preparation. It wasn't that we couldn't count them or classify them; we couldn't make the preparation enabling us to work. I didn't solve that.

Jay Minkler and I were so disturbed; we just couldn't figure out why it didn't work. We went back and combed the literature and in obscure places we found others reporting that they could never get good chromosome preparations out of fresh cancer. I went back to the NCI and I saw Katherine [Koontz] Sanford, who had done an awful lot of work on cultures of cells, and told her my problem. She said, "Yes, I know about that problem. We've never succeeded any better with that." Still seems to be a problem.

Because of that I'm loath to take our work as seriously as I would take it because it's a biased sample. We have a sample of those things that would grow well. And until and unless I could get an unbiased sample of getting, say, 90 percent of the cancers to grow well and be able to analyze them, I think it remains moot as to whether our findings are generalizable.

Hughes: Has anybody carried on that work?

Gofman: No, not in a quantitative fashion. As a matter of fact, it would be even better to do now than when we quit. At the time we quit, when I gave up the project, Cassperson had just recently introduced his methods of what's called banding techniques, whereby you can study the chromosomes not only for length and position of the arms versus the thing called the centromere, but also you could see detail along the arms. You could really tell now whether an arm is really that chromosome or has been broken off and translocated. So there has been an order-of-magnitude step forward. If we would have added the banding to our technique of analysis, I think one could study the question more effectively than ever.

I'm just disappointed. I just looked at the literature recently and found nobody has taken up that quantitative approach. We had a really beautiful quantitative approach, assisted elegantly by the electronic engineers with their scanning, and Stuart Stone, who developed the computer program for reading our drawings.

See, Livermore was a great place to work. There were people with lots of talents there in these highly technical areas. Oh, I just periodically think maybe I ought to try to get back in and get into that work itself. So far as I know, the problem of culturing the cells or getting good preparations from the cancers has not been solved any better. But the chromosome analysis could be better now. But I haven't seen anyone do it. I think it's valid and should be done, but nobody seems to have done it. Now, it might be going on and I don't know about it, but I haven't seen it.

Hughes: Well, the textbook pictures that any biology student has seen, the chromosomes lined up on the--

Gofman: Metaphase plate.

Hughes: Metaphase plate--I couldn't think of the term there--are those photographs usually taken of cell cultures?

Gofman: Yes, most always cell cultures, not direct material. What you do is, you take blood and you add some plant materials. One of them is called phytohemagglutinin; it's an extract of plants and for reasons that are not clear it stimulates the lymphocytes of the blood to go into a lymphoblast stage and to grow.

They do grow and divide and after culturing for 48 hours or 72 hours, you then arrest the cells that are metaphase with either a compound known as colchicin, or vinblastin. They just stop the thing in metaphase and you accumulate things in metaphase. Than you swell the cells by putting them in a dilute solution and then spread them on a clean slide after killing them and fixing them with a 3-to-1 mixture of alcohol and acetic acid. You have to use very clean slides, and they spread out beautifully.

The other way is: We'd make a small biopsy of the tissue, just a little beneath the skin, and get some fibroblasts that way. We would put little bits into a culture bottle, feeding them, then they would grow out and fill the whole bottle. Then we discard the culture medium and get the cells off, again swell them, stop them in metaphase with colchicin, and spread those. So what you see mostly in pictures are either fibroblasts or peripheral blood lymphocytes that have been grown in culture first.

We'd just take some of our fresh cancers and tease them apart and shake them so the cells would come loose. Then we would go through this procedure of stopping them in metaphase and swelling them and killing them with methanol acetic acid and putting them on a slide. Occasionally we'd get some spread-out metaphases, but they never looked like the kind you'd put in a book. I don't know why.

Hughes: So it's across the board with fresh tissue in general....

Gofman: Seems to be fresh tissue in general that you can't get as good preparation. Maybe it's even worse with the cancer cells. I'd have to look carefully to see how many good metaphases we ever got on our cancers that had 46 chromosomes and could have been normal cells. But it's a difficulty.

Low-Level Radiation

What happened when we came out with some facts about cancer and radiation: Within two weeks certain officials of the AEC, not Glenn Seaborg, were denigrating our work publicly, saying to reporters that we were wrong, that we were incompetent. It was a most interesting situation. Here is the department of the AEC that had just awarded seven years of $3-to-3.5-million budgets to be used under my general guidance, since I was the associate director for biology and medicine at Livermore. And two weeks after we'd come out with a paper on radiation, cancer, and chromosomes--By the way, it was an invited paper from the Institute of Electrical and Electronics Engineers. In two weeks we became incompetent. Here's somebody that for seven years gave me $3.5 million a year and couldn't detect my incompetence; in two weeks I was incompetent.

In that first four weeks, I had a call from a fellow by the name of Frank Howard, of Newhouse News Service, and he said, "I have some questions to ask you. I've been talking to Dr. John Totter of the Division of Biology and Medicine of the AEC and he said your work on radiation and cancer isn't really directed toward cancer at all." I said, "Well, that's interesting. What is it directed toward?" He said, "Well, what you're trying to do is undermine the national defense"--which is a strange thing. I have a pretty good record of what I've done for the national defense.

I said, "What do you want me to do?" He said, "Well, what do you have to say about Dr. Totter's statement about undermining the national defense?" I said, "I have nothing to say. I don't think I would stoop to that level to even try to answer that sort of charge." He said, "You're not going to deny it?" I said, "No, I won't deny that. Why don't you go ahead and publish it if you think it's a credible report?"

He said, "I'm very worried about you." I said, "What do you mean?" He said, "If I publish it, then I'm the conduit of that information, and if it's false, you could sue me for libel." I said, "Mr. Howard, you're a journalist. You write what you want to write and if you say something about me and it's libelous, I'll do what I want to do. But I don't have to affirm or deny your stupid charges. You just go ahead and do what you wish to do." And that's where we left it.

Of course he never published it. But it's an illustration of what was going on. About two to four weeks after that, I was very upset about the thing, not too surprised, but upset, so I wrote Glenn Seaborg a letter. Somewhere that letter exists, possibly in my files here and maybe in his. But I said, "I want you to know that I am not endeavoring to hurt any Atomic Energy Commission program with this."

[interruption]

That was very early '70 I sent the letter. I said, "Glenn, this issue is exactly what we were put out here at Livermore to find out about, the effect of radiation. If the cancer hazard is, as we find it to be from human data, about 20 times as bad as people have thought and no suggestion of a safe threshold, I think that the Atomic Energy Commission program would benefit greatly by knowing these facts and planning accordingly rather than trying to fight the facts."

So I said, "It is my opinion, Glenn, that members of your staff, such as Dr. Totter and others, are serving the country and the Commission very poorly by attempting to destroy us personally rather than answering the scientific facts on their merits. I would urge you to do something about it." And he just wrote back and said it was just something he couldn't get in the middle of. It was a matter of a dispute between his staff people and me and he couldn't, as a commissioner, get into it. He didn't want to.

There was one series on CBS News; five mornings in a row 15 minutes of the morning news were given to this thing where Tamplin and I were given a lot of time and Glenn Seaborg was asked some questions. The one thing he said on the CBS News morning report was, "I think there is a safe threshold of radiation." But that's all he said.

In numerous interviews with newspaper people and TV and radio people, when asked about my work, he repeatedly said, "John Gofman is a fine scientist and I really cannot say. It's not my field of expertise, but I do have a very high regard for him as a scientist." He never once attacked us or suggested that this might be less than good science, though on that one program he said [he thought there was] a safe threshold. [Seaborg] never once made any derogatory remarks about us as scientists that ever came to my attention.

In contrast, and I have no reason to say that there was any collusion between Seaborg and John Totter--I think Totter did it on his own probably and maybe thought he was endearing himself to his superiors--John Totter conducted a vigorous campaign of slander and smear against us. Even worse than that, he made an ass out of himself. I can say that very clearly and appropriately because it's a matter of record. He was interviewed by a reporter for the Los Angeles Times--I believe it was the Times; it was one of the Los Angeles papers--in '71, I think it was, or late '70. He was asked what he thought about John Gofman's work on radiation and cancer. Dr. Totter, the head of biology and medicine of AEC, said, "Well, Dr. Gofman's work on radiation and cancer is about as good as his work on cholesterol and heart disease, and you know how good that is."

Well, the reason I say he made an absolute ass of himself was that within a matter of a few months, it was announced publicly that I was sharing the 1971-72 Stouffer Prize for my research on lipoproteins and heart disease. So what he did was to lend great credibility to our cancer work by that stupid remark. But I don't think any of the commissioners put John Totter up to it.

There is a more disturbing thing, which I think for posterity ought to be recorded, and I can only say it was alleged to me. But about the same time, I got a call from a man in the U.S. Public Health Service involved in radiation health. By the way, we had sent out our findings and we kept sending them out in these G-T [Gofman-Tamplin] reports to about 100 leading scientists in the country, because it was becoming apparent that this was going to be a war, not a scientific dispute. And we wanted others to have the data, at least 100 of them. We carefully selected a list that we sent them to.

One of them was a scientist in the Public Health Service. He called me up in early '70 at my home and said, "I have something very unpleasant to say to you, but I think I have to tell you." I said, "Well, that's strange." He said, "Further, I want to say that if you ever want to use this information in any hearing or court, I will deny it." I said, "That makes it almost useless, but it's your nickel. You called me. If you want to tell me, go ahead."

He said, "Well, I'm a neighbor of Dr. Totter's and he came over to my house on Saturday and said, 'You've got to help us destroy Gofman and Tamplin.' I was shocked by hearing these words and I said, 'What do you mean? About their stuff on radiation and cancer?' And Dr. Totter said yes."

This Public Health man says he responded to Totter with these words: "They sent me a copy of their paper, their report. I've gone over it. While I don't agree with each of their numbers in detail, it looks like the general thrust of their calculations is correct. The details on whether this number should be a little higher or lower is something nobody can say, but it seemed reasonable."

And then came the shocker. He said Dr. Totter said to [him], "It's not a matter of whether Gofman and Tamplin are correct or not; the issue is that if their work gains credence, the nuclear energy program will be destroyed, and right now we need the nuclear energy program. Besides, by the time the cancers and leukemias occur that they're talking about, you'll be retired and I'll be retired, so what difference does it make?"

So he said, "I thought you ought to know this, but I'll deny it." This man is now a professor at the University of Illinois and he may be willing to open up and confirm it. But that was another indication of what we were dealing with. And there are many unpleasant illustrations of that.

Talking about the Livermore Lab, Johnny Foster had gone on to Washington several years before and Michael May, for whom I had a high regard and a very good working relationship with, was then the director. Roger Batzel was still associate director for chemistry. Oh, as a matter of fact, Roger Batzel had taken on my job as associate director for biomed about six months before, because I was so up to my ears in the research in the laboratory that I'd appealed to Mike a year before to be relieved of the associate directorship. He said, "I just think it'd be good if you waited a while." So I said, "I'm real excited about the research program in the lab and I just don't want to divert from it." So he got me to defer it for a year. And then I asked him again a year later, in April '69, and he said okay. So Roger took on my duties as associate director for biomed. Roger during that period served as sort of the catch-all for any of Mike May's problems as director.

And here was a problem, when this whole thing came out. Roger was over in my office. He went over all the calculations and he didn't have anything to say that we were wrong. In fact, Paul Jacobs, the radical journalist who died recently--you may have seen Paul Jacobs and the "Nuclear Gang"--Paul Jacobs went out and interviewed Roger Batzel. This was put on the air, and I heard the tape. I don't know who has that tape. "Have you ever found any fault with Gofman's and Tamplin's calculations?" And Roger Batzel said no. He said, "Have you gone over the calculations?" And Roger Batzel said, "Yes, and I don't see anything erroneous in the calculations, but I disagree with their position on nuclear energy."

So I had some very rough sessions with Roger Batzel in that period. We were very close and I just said, "I think your behavior is despicable, and the whole board of directors', in not backing me, as that was the agreement originally."

Hughes: Could he account for that stand?

Gofman: No. I just said, "Roger, your behavior is not any different from just a straight out-and-out prostitute." Roger Batzel sat in my office in tears when I castigated him because, frankly, he knew just what he was doing. He and Mike May sensed, my God, if we let this flap blow up, the Lab will be in trouble for money. Money is the thing. It was far better to sacrifice scientific honesty. Well, I wasn't being sacrificed in any sense. You couldn't say that I was suffering. First of all, they didn't fire me. But that's not the issue. I had a professorship to go back to any time I wanted, so you couldn't say...

Hughes: Do you think that made a difference to you?

Gofman: No. I would have done the same thing; didn't make a difference to me. But you could say from the outside that I did have something to go back to and therefore wasn't being sacrificed or losing my livelihood. And for that matter, even if I didn't have the professorship, I never worried about being able to make a living. I could always practice medicine as a last resort. No, I don't think it made a difference to me, but you couldn't say they were putting me out in the cold, so to speak. They did not fire me. But they did decide to sacrifice scientific honesty.

I'll tell one thing in their favor that happened a little bit later in that controversy. Mike May and I had a run-in, which you may have read [about]. I think I wrote it up in Poisoned Power. I had an invitation before the controversy to go speak at the December 1969 American Association for the Advancement of Science meeting. It was to be a meeting on nuclear energy. When they invited me--this is before we gave our controversial paper--I said, "Hey, the area you want covered is really the area that Arthur Tamplin has been working on with me for seven years. He'd be far better at this meeting than I would. Why don't you have him come instead of me?" And they said fine.

So this was set up in May and Art was happy to do it. Well, two or three weeks after we gave the paper at the Institute of Electrical and Electronics Engineers--that was November of '69--it turned out, wholly for other reasons, Senator Muskie invited me to testify before the subcommittee on air and water pollution. I gave essentially the paper we had done at the IEEE, but under the title of "Federal Radiation Council Guidelines: Protection or Disaster?"

Hughes: Do you think it was the IEEE paper that had brought you to the attention of Muskie?

Gofman: No, it turned out not. I thought it was. It turned out that one of his staff people had noted that I was associated with the Livermore Lab, involved in this whole business of Ploughshare, underground testing, and had recommended my name. Didn't even know about the IEEE paper. And this staff person, I later heard, was shocked by what I said because the last thing he was after was rocking any boats. And so I gave this thing on "Protection or Disaster?" which amplified what was said at the IEEE a little bit: the same message, with a little more substantiation.

I remember one thing Senator Muskie asked me in the questions and answers: "Well, doesn't coal kill people, too?" And I said, "Oh yes, and I would like to say, Senator Muskie, that I do not condone homicide with knives any more than guns." We had a good exchange at that meeting. Then things were rough.

Hughes: You mean specifically after the Muskie...?

Gofman: From that day forward, yes. Edward Bauser, who was the staff aide to the Joint Committee on Atomic Energy--that's the all-powerful committee that used to exist on all atomic energy matters--came up to me when I sat down from giving my testimony. He said, "Mr. Holifield, the chairman of the committee, wonders if you could come over this afternoon to talk with him." I said, "Well, I'll be in town all day. I certainly can. Art Tamplin is here in Washington too." He said that'd be fine if he'd come too. So we went over to the Joint Committee offices at two o'clock. It's a secret room in the Capitol Bulding. Because they had so much classified stuff, they had a classified area within the U.S. Capitol.

Hughes: I didn't know that.

Gofman: Yes. We sat down after being ushered in and in came a few aides, Representative Craig Hosmer of California, Chet Holifield, then chairman of the Joint Committee, and Representative Melvin Price.

"Just what the hell do you two think you're doing, trying to destroy the nuclear energy program?" And that was the beginning of the conversation. So I said, "Well, just a minute, Mr. Holifield. I don't think that's a correct characterization. We are putting out some information based upon the research we were asked to do on the radiation hazard of developing cancer and leukemia." And he said, "Oh, never mind all that. You're just going to have every little old lady in tennis shoes in this country up in arms against the nuclear energy program."

A guy by the name of Graham, one of the staff aides, said, "Well, Mr. Holifield, these are two very reputable scientists from our own Livermore Lab that we put in charge of this program." He said, "I don't give a damn about who they are. They're trying to destroy the nuclear energy program." And he leaned over and said to me, "Listen, I've been told that at a hundred times the allowable dose, nobody's going to get cancer." I said, "Who told you that, Mr. Holifield? I don't know that that's true, and my research would indicate otherwise." He said, "The AEC officials told me that." I said, "I just think they're wrong, but I'll try to write you a more detailed report about what evidence they're using to suggest that."

He got even wilder; he looked at Tamplin and me; he leaned over the table and said, "Listen, there have been others who have tried to destroy the AEC's programs before. We got them and we'll get you." That's the Congress of the United States. Freedom, democracy, scientific honesty. "We got them and we'll get you."

Well, when Arthur and I got on the plane, we realized what was involved in this whole thing now. So it was just a question of how long you could survive and get the truth out and how best to survive. That was where we got this idea of sending out the papers to 100 people.

Hughes: You didn't have any second thoughts at that stage?

Gofman: No, no, there was no turning around at all. But I wasn't back about a week when Mike May called me into his office--he was then director of the Livermore Lab--and said, "John, I have a request to make." I said, "What is it, Mike?" He said, "The AEC people contacted me and they're very upset about the paper you gave at the IEEE and this testimony you gave before Muskie." I said, "What do you mean, they're upset, Mike? You mean they would like me not to be allowed to give these papers? You know that's absurd."

He said, "Oh, I recognize that's absurd. That's not what they're upset about. They're upset that you've given these things without them having any prior notice, so they don't know how to cope with the press or the media people who ask them about it." So I said, "Oh, I have no objection to giving them things in advance of presentation, but you know as well as I do that sometimes you have your paper ready the night before you give it." You don't really always have things weeks in advance.

But I said, "To the extent that we have things ready weeks in advance, I'd be delighted to provide them with a copy of it and I'm sure Arthur Tamplin would." So I said, "Of course, Mike, you realize that any tampering with any of the papers because they don't like it isn't going to be tolerated by me." He said, "Do you think it would be tolerated by me?" I said, "No, Mike. I don't really think you would tolerate it."

Well, that was very reassuring, that I was going to have support at the Lab. I had such a good relationship with Mike May and thought so highly of him that it didn't surprise me. I went back and talked with Arthur Tamplin, said they would like to have papers in advance so they could show them to the AEC. He said, "I don't have any objection to that."

So it turned out that this AAAS meeting was to come up about three weeks later, the one that I had recommended that Arthur talk at instead of me. Art said, "I've got my paper just about ready." I said, "Fine. Give it to me and I'll take it over to Mike's office." So I did, and about two days later, Arthur Tamplin came into my office and he was just white with anger; threw the paper down on my desk and said, "Roger Batzel just brought this back and Roger told me that if I insist on giving the paper as it is, that A, I cannot identify myself as coming from the Livermore Lab, B, I must travel on my own funds--I cannot use Lab funds for travel which would otherwise be paid, C, I must not use any Laboratory secretary or Laboratory facilities for either typing or duplicating the paper."

So I looked at the paper and I've described it some place as having had everything deleted but the prepositions and conjunctions. I still have the original paper with all the pencil marks of Roger Batzel. I said, "Well, we take it from there, Art. It's going to blow up or we'll find out."

So I went over to Mike's office with this paper and I said, "This is totally at variance with what we just said about a week ago about no, you wouldn't tolerate censorship." He said, "For heaven's sake, Jack, why don't you be realistic?" So I said, "Realistic--I'm very realistic. This is just intolerable. I don't intend to stand for censorship." And he said, "You're just overwrought. You're not being realistic."

I said, "Look, I'm going to call up the guy at the Miter Corporation who invited me and I'm going to tell him that Tamplin can't come to that meeting because the Lawrence Livermore Laboratory practices censorship, and that I want him to read my letter to the assembled AAAS meeting because I was the one that arranged for Tamplin to come. Therefore I feel it's my duty to explain why he isn't coming to present this paper." He said, "Would you do me a favor? Would you just go home and sleep on it? You're just upset." And I said, "Mike, I'm really very cool, but if you want me just to leave, I'm going to go ahead and do just what I said I was going to do." So he said, "Okay, we'll talk about it tomorrow."

I went back to my office, called the guy at the Miter Corporation, told him exactly what the circumstances were and that I was going to write this letter to him. He was very upset because he didn't want to be in a flap with his meeting that he was arranging: "Oh, I certainly hope you can work it out. Very disturbing."

Hughes: Very disturbing because he wasn't going to have a speaker?

Gofman: No, that there was going to be this letter coming in that I wanted read to the assembled AAAS. He had visions of reporters commenting, and here he was the chairman of this session. So the next day Mike came into my office, all smiles. He said, "Well, did you think it over?" I said, "Yes, I did, Mike. I called the guy I told you I was going to call." And he said, "You did?" I said, "Yes, I told you I was going to do it when I left your office." "Well, what did you tell him?" I said, "I told him the Livermore Lab was a scientific whorehouse and that they practice censorship and that I was going to have a letter to that effect that he could read at the AAAS meeting." Mike just became furious and turned right around and stormed out of my office. We spoke once after that, but that's all.

Hughes: Then what happened?

Gofman: What happened was that he was apparently very worried about that. So Roger Batzel came back to Tamplin and was suggesting a gross modification of his censorship with just a couple of statements changed a little from what Art wanted to give. Then the secretaries could type it and he could have his funds to go and identify himself as from the Lab. Tamplin was quite happy about the two sentences that they wanted really changed. So they backed way down and Tamplin went and gave the paper and my letter was not sent. But it was the end of my relationship with Mike May.

Hughes: What about other people there? There must have been a considerable strain at the Lab, for everybody concerned.

Gofman: Yes, it was quite a strain. The interesting thing was that Roger kept coming to see me. We never broke off our conversations, although, as I say, on at least that one occasion he was in tears in my office, when I castigated him for his behavior. But we did continue to talk and he did continue to come to visit me.

But interestingly enough, in my own department --see, in '63 I became the head of the project and associate director, and in '64 I asked Johnny Foster, by this agreement that I'd only have to do it one year, could I now give up the chairmanship of the department. He thought that would be a terrible idea. I said, "Well, I thought it was only a year." He said, "Why don't you wait a while?" In '65 I asked again, and late in '65 he agreed that it would be okay if Dr. Bernard Shore replaced me as chairman of the department so I could go back in the lab and work. But I remained associate director of the Laboratory and as such was in charge of all things pertaining to biology and medicine for a while, as one of the nine associate directors. So Shore was by then the head of the department.

But in the period from '63 to '65, as head of the department, I approved or brought all the scientists that came, some thirty-odd M.D. or Ph.D. scientists that we had. Then as associate director for the next several years, when Shore thought of somebody to add, we discussed it and decided together whether we should get that scientist if he'd come. So either directly or indirectly I had chosen all the scientists in the department.

But once it became known that there was this controversy, none of the scientists in my own department would come into my office. I didn't have any argument with any of them. At the times when I was in the lab working, nobody would ever come in and bother me because they knew I just wanted to work. But the times I was in my office, I always had a very open-door policy. If they wanted to come see me they didn't have to make appointments. If I was not doing something with someone, I'd be happy to see them. The scientists in that division were in and out of my office all the time, either to discuss their research or their salary or their personal problems or whatever.

Almost all of them in the course of time would be in and out of my office, some more often than others. But once that got known about this argument, my office was like a graveyard as far as any scientists coming to see me. Nobody would be seen [with me] besides the few that were working under my direction in the lab. Nobody came to see me.

Art and I were working very hard. We were at the Lab until midnight. Often I slept over at the Lab because we were putting out additional supplemental material in these reports that are now called the G-T reports. It was all done in a course of about six weeks, 180 or 200 pages of scientific documentation. In the course of staying there late a lot, on two separate occasions two scientists each poked his head in and said something to the effect of "You know, I agree with what your papers show on this stuff." I said, "Oh, that's interesting. Art and I are terribly busy. Would you like to help, because we have several areas of the work that we have not gotten to?"

The answer in both cases was, paraphrased a little, essentially this: "Look, John, you're a professor. You've got a lot of prestige in the scientific community. You can get away with this. I can't." One of them I remember saying, "I've got a mortgage on a house I just bought. I've got two children and a wife and I just can't afford to have my throat slashed." I think they were stating the situation very, very clearly and correctly. Well, we certainly did enough, as Mr. Holifield said, to have them want to get us.

Hughes: What about your role in the eventual demise of the AEC?

Gofman: I think the fact that the AEC could no longer be tolerated as a national organization of government, the words AEC--that is, the agency persists in new form, but the words could no longer be tolerated for two reasons. The first was the enormous loss of credibility they had in the controversy with us. Well, I'll state that and tell you how that all came about.

The second was as a follow-on to the controversy [involving us] where they handled it in just about as inappropriate a fashion. That was in 1971, when Professor Henry Kendall, at the Massachusetts Institute of Technology, and his colleague, this young economist Daniel Ford, came out with a challenge to the emergency cooling system of the nuclear power plants; said it might not be adequate, might not function at all. The AEC handled them in exactly the same fashion--attempted to destroy their personal credibility on a personal attack basis. But they made a very bad judgment in that too because Henry Kendall is a man of very great depth. They held a series of hearings in 1972 at Bethesda to show how stupid Henry Kendall was. They had him on the witness stand for weeks and he made a fool of them, made an utter fool of the Atomic Energy Commission.

In one part of these hearings, Dan Ford was doing some of the cross-examining of the AEC staff. He had Milton Shaw, the director of the reactor program, on the stand and he read him some things and Shaw said, "I certainly wish you'd define your terms so I could answer your questions." And Dan Ford said, "Well, I'm just reading from a report that carries your signature as the author." So the Atomic Energy Commission, with reporters there, came off badly in those two controversies. The emergency core cooling system controversy had never been resolved, still exists. Ford and Kendall built a Union of Concerned Scientists on that controversy. And that I think was a follow-on to ours, meant the credibility of the AEC was at zero.

There was a third thing that happened. Mike Gravel wrote a letter to Robert Finch, then secretary of HEW. Gravel was the actual man on Muskie's subcommittee who had issued the invitation for me to testify. He stayed only for part of the hearing. They wrote me a nice letter after. I later got to know him quite well. Mike wrote a letter to Finch and asked what he was going to do about the Gofman-Tamplin testimony. At the same time, Ralph Nader asked Muskie what he was going to do about it, because Nader got alerted to this. I'd never met him before and he sort of got into the act. He later became an avid anti-nuclear spokesman after Henry Kendall's attack on the emergency core cooling system.

With Nader asking Muskie and Gravel asking Finch, Finch responded and said that he would ask the National Academy of Sciences to set up a committee to investigate our charges. Now that's interesting. I have no respect for the National

Academy of Sciences, I want you to know, because I do not think they handle anything well. I think the National Academy of Sciences is generally used to try to put down things that the government doesn't like, by appointing a committee to find that whoever's report they have to investigate is not correct. That's sad, but...

Hughes: Why is that?

Gofman: It's a government funded organization. It's quasi-governmental, but it's government funded. If you look at thir funds, you'll find they're virtually all from government. That's sad because the whole idea of the Academy is to serve as an independent agency. Perhaps in some controversies it may very well have done an elegant job of really serving that way, but in some that I've seen I have no respect for it.

Well, they appointed a thing called the Committee on the Biological Effects of Ionizing Radiation. The reason why I have no respect for the committee and the National Academy was that in its entire existence and having been appointed to investigate our findings, there has never been one word of communication from that committee to either Tamplin or me. We were not invited to be on the committee, a committee of 60 scientists to evaluate radiation, appointed for the sole purpose of checking our work. We were not invited, nor were we ever queried. Now, in their reports in 1972, they make extensive references to our documents. Well, it is clear what was wanted of the Academy was a whitewash.

In their 1972 report, however--and I think this was another nail in the coffin of the AEC--the

AEC was in effect saying we're talking about 32,000 people per year dying of cancer from the existing standards. Some of the AEC staff people said it won't be 32,000, wouldn't be three, wouldn't even be one. Gofman and Tamplin are wrong by 10,000-fold. The BEIR committee said the proper number was somewhere between 3,000 and 15,000. Our number was 32,000, with a best estimate of about 6,000. The five-fold difference they got from us--they said they were five-fold different--turned out to rest on a thing that today... In my book you'll see it, when it comes out. They're going to have to eat crow because they made a five-fold mistake. They said we made a mistake, but we were just on the beam.

In the 1979 report, they're now back to the numbers we used on that particular thing, but they don't even say a word about the fact they made that five-fold blunder. That was the way they got a number lower than us, but that wasn't the point. The point was, in the world scientific community we were regarded as totally vindicated by the BEIR report because scientists knew that the uncertainty in these numbers is such that if we say 32,000 and somebody else says 3,000 or 15,000 it's the same ballpark. It isn't three or one as the AEC was saying.

So I believe the answer to your question is, there were three things. The controversy with us, an even bigger flap in the controversy over emergency core cooling safety and their incompetence in handling that with Kendall, and then the BEIR report essentially confirming [our figures]. I think it was time that [name] disappeared: AEC.

Hughes: Was that enough, though? It seems such a superficial change. I know you've said, and the facts stand, that it was merely a shifting of hats, so to speak.

Gofman: Oh, sure.

Hughes: What was really accomplished by--?

Gofman: Nothing, nothing. As things have turned out, very clearly nothing. They took all those aspects involved in regulating nuclear power and called them the Nuclear Regulatory Commission, and the rest became the Energy Research and Development Agency. Some people asked me, "Do you think this is going to be any better or really improve it?" I said, "It's too early to say. I really can't comment and I think I'll reserve my judgment until after they've had some opportunity." I might have said I was skeptical, but I did not comment forthrightly.

But after about a year and a half, I started to comment quite clearly that I thought it was just all a sham. You don't need me to confirm that. The history of what happened with the Nuclear Regulatory Commission in the aftermath of Three Mile Island shows what a sham it was. They have just been raked over the coals completely for the fact that they were not a regulatory agency but a nuclear promotional agency, which was the whole reason for breaking them off from the AEC.

Hughes: Was that the overriding excuse that people gave for wiping out the AEC, the fact that the two functions should be separated?

Gofman: That's the excuse they used. It was only for the division of the functions, that the Energy Research and Development Agency could sponsor the promotion of the various technologies for producing energy, but it was unconscionable to have a government agency that was sponsoring it and also doing the regulating. But if they divided it, they'd have the protectors over here and those who would promote the technology [over there], and they'd be separate, which turned out to be a joke in retrospect.

Hughes: I believe that one of the things that came out of that BEIR report was an initial support for the linearity hypothesis, by a bare majority, and then they shifted their view.

Gofman: The 1972 committee supported the linear hypothesis. The 1979 revision committee had a majority supporting it and a minority not supporting it. Could you wait about five seconds, and I'll give you a very interesting fill-in on that? Let me just get it.

[pause]

You're not going to believe this, because the statements I have made about the National Academy of Sciences and the BEIR committee turn out to be mild compared to what's coming out now as a result of this controversy over linearity and nonlinearity.

An article by Mark Bowden of the Knight News Service quotes Dr. Edward Radford stating the following: "When the BEIR committee first came out with the linear model in '72, it was just a theory." Of course it wasn't a theory originated by them. "We were taking high dose data from Hiroshima and Nagasaki and extrapolating downward. But with more evidence in the BEIR II study, we began to see the theory proving out. With the BEIR III study, the evidence was even more conclusive. Rossi, whose field of expertise is physics, interprets the Japanese data differently. He insists that it supports the quadratic model, the gradual sloping curve that predicts a relatively safe region for weak radiation exposure. Rossi feels the data strongly support his own theories on radiobiology, and his model is correct."

Now listen to this. "Dr. Rossi's opinion has already been refuted by the evidence, Dr. Radford insisted." I might interject that Dr. Radford is the chairman of the BEIR committee. And here's what he says further. This is now not John Gofman, but Dr. Edward Radford, the chairman of the BEIR committee. "In my opinion, most people who support his position"--that's Rossi's position--"are employed in the field of nuclear medicine or radiology, which means they have a strong personal stake in seeing the dose risk estimates lowered. Putting these kinds of people on a committee charged with assessing public health hazards is like putting a fox in to guard a henhouse."

If I have said anything about the BEIR committee, about government, about the fraud to which the American public is being subjected, my statements are as of the mildest compared to that statement of Dr. Radford's.

Hughes: It's hard to believe.

Gofman: Hard to believe, and as a citizen of the republic, I would think that people would be so incensed that their very life--whether they and their children will even have a life--will be manipulated by people for personal vested interest. I would think there would be millions of people in this country ready to tear certain individuals limb from limb, who are defrauding them of their basic human rights and of life itself.

Radiation Effects Research

What I was doing just scared the daylights out of everybody at the Livermore Lab and out of my former colleagues in the Donner Lab. A sad true fact about scientists, and probably the most scathing condemnation of scientists I can think of, is that, as a general group, they will sacrifice any principle or people for their grants and positions. That's probably not indigenous to scientists, but it's very blatant in view of the fact that, in contrast to many other fields of scholarly endeavor, scientists have probably, I would say, a ratio on the average of at least 10, maybe 30 to 1 of money support per person. It takes equipment, people to carry through experimental work. So they are very fearful.

So when you ask were some of my colleagues at Donner aware of what I was doing in the early 70s, I think they would have preferred that they could somehow be made totally unaware of what I was doing. They just didn't want to know, because one of the inconveniences for a scientist is that if he knows the truth and then doesn't do something about it, that is an even more serious condemnation. But if he doesn't know the truth he can at least say, "I haven't looked into that subject." And he does a damn good job of not looking into it if it would be something that might embarrass the source of his funds. Sad, but true.

Hughes: There were some experiments going on in Donner Lab which were right down the alley of what you were saying--experimenting with the effect of various forms of radiation on yeast, on all kinds of unicellular things.

Gofman: They stayed away very carefully from the human evidence. That is an intriguing story of itself, both at Donner and elsewhere. It's part of what I just got through saying--if you don't know the truth. You can be doing intricate, sophisticated, detailed studies, so long as you're restricted to yeast, bacteria, mice, fruit flies, and never say anything about humans. You get nobody in any trouble. What the Atomic Energy Commission just never wanted was any information about humans. If you study carefully the record of the Atomic Energy Commission, you'll find how carefully they avoided getting information on humans. But they do a lot of work on animals.

Hughes: Well, in a sense they do work on humans, too. I'm thinking back now to the early neutron therapy trials, which I know preceded your arrival at Donner Lab.

Gofman: Therapy trials hurt nothing. See, the thing that's sensitive is the dose that people might get from any nuclear program: weapons testing, peaceful uses of explosives, nuclear power, or the dose that workers might get. These are sensitive areas. Therapy is sort of like the good guy. The AEC and its follow-on, the Energy Research Development Agency and the Department of Energy, get only kudos for what they can accomplish with therapy, because that represents a beneficial by-product of their entire activity, and all they can hope for there is cheers.

But fallout of weapons testing on Utah residents causing leukemia and cancer, or workers in the nuclear industry or in labs getting cancers or cataracts or leukemia, or people getting irradiated from nuclear power--that doesn't bring kudos; that may bring the end of the program. So that's why you see this very great difference in their support of therapy versus finding out things about humans.

Hughes: Even therapy in those early days ran into trouble. Remember Robert Stone stopped it, because there were all kinds of awful side effects? I've seen some pictures--ulcers that didn't heal. I don't remember any talk of induced cancer, but when later your studies and others' came along, how could a person help making a correlation there?

Gofman: Well, they have one thing to hide behind, and it's the last crutch that's available now. In the book I just completed, Radiation and Human Health, I think I've removed that crutch effectively. And that is, they said, "Well, everything you say is true, but it's all at high doses. At low doses it isn't going to happen." I watched this evolution--very interesting.

The earliest data on leukemia from Hiroshima and Nagasaki necessarily showed that leukemia was occurring at a hundred rads or more body exposure. Well, the reason why you'll always see it in the high dose cases is because if the risk goes up in any fashion with the dose, you can expect that those with the highest dose will show earlier statistically. I remember the AEC publication saying this suggested that the threshold must be below a hundred rads. At that time, I even thought that was possible, that there was a threshold. Then, as more data from Hiroshima and Nagasaki came in, they said, well, the threshold must be below 50 rads. Then below 20 rads...

By the way, now out of Hiroshima and Nagasaki the work has come pretty full circle. It turns out that breast cancer cases are in abundance in Hiroshima and Nagasaki. Even the establishment scientists like Dr. Land from the National Cancer Institute and McGregor have published a 1979 paper showing that breast cancer is a straight-line proportionality to dose, even down to the low doses in the neighborhood of 2 to 15 rads at Hiroshima and Nagasaki. Now this straight-line relationship, instead of the hope that there was a threshold, is a disaster for atomic energy in all its phases.

My only concern is that I would stop a large part of the activity today if it were my choice. The basis for that statement is a very simple one: I am convinced that the uses of nuclear energy by society are going to decrease astronomically when society wakes up. And the only thing that's involved, as I see it, is whether we have to kill a hundred million people or 50 million or 10 million or 500,000 before that happens. But the key thing about it is that this one crutch they've hidden behind--maybe there's a threshold; maybe at low doses it won't be as bad--all that is disappearing day by day, including the leukemia evidence, which already disappeared, and the Hiroshima and Nagasaki breast cancer evidence.

Hughes; You state many times in your works the fact that Dr. Alice Stewart 20 years ago found a linear relationship going way down to low doses....

Gofman: Even to a quarter of a rad.

Hughes: What did people do with that information?

Gofman: Well, that is an interesting story. It is exactly correct that Alice Stewart first published in 1956 a preliminary study, which is now famously known as the "Oxford Study," showing that children who died before age 10 of leukemia or cancer had mothers who had about a 50 percent excess of radiation in their history, compared to normal children. Subsequently Dr. Stewart showed that not only does this prove a risk of cancer and leukemia in the child, but she showed with her colleague Dr. George Kneale that if you go from one to two to three to four to five x-ray films, the risk of leukemia or cancer in the child goes up proportionally. [This] puts the linear proof way below the Hiroshima-Nagasaki evidence, a quarter to one-and-a-half rads.

The medical community laughed at it for years and said it couldn't be true; we've been irradiating people and we don't see cancer. There's a delusion that goes on in the minds of doctors that is based upon the fact that they don't want to think about the harm they're doing. Being in the medical profession, I know just what the psychology is. It's hard to think that you're doing something that may kill people.

For example, in the 50s, when Hammond and Horn came out with their study on cigarettes and the induction of lung cancer, I went to many medical meetings and watched professors and leading physicians get up and say, "I've treated smokers for years and I've seen no evidence at all that cigarettes cause cancer." Now it is well established in everybody's head that 90 percent of lung cancers come from cigarette smoking. But just as well as it is established now, when Hammond and Horn came out with their first report, physicians got up publicly in meetings [and said], "I've been in medical practice for thirty years and I've never seen a lung cancer caused by cigarette smoking."

So what they said was, "Well, I've been giving women x-rays and I've never seen it cause cancer. They just never did the requisite studies the way Alice Stewart did them. Well, McGregor and Newcomb have restudied her data and they've confirmed the relationship is a linear association [between] dose and effect. Holford has published a paper reanalyzing the Stewart data with the same thing. In this country, McMahon at Harvard did a very large study where, instead of using the history of x-rays recalled by the mother--which was one criticism of Stewart's study, which I think she answered, actually--McMahon used hospital records to estimate the dose, which weren't subject to recall. And in a very large study he confirmed the Stewart findings, which was very embarrassing.

But then the shift was: Well, maybe it is true that there is a greater radiation history in the mothers of those who have [children with] leukemia or cancer than in the others. But, they said, it's probably not causal. The way it works is that these women have a certain constitution that leads them to have x-rays during the pregnancy and wholly separately to have [children with] leukemia or cancer. You can't say it isn't possible that that's true; it's just remote.

But I think, as Alice Stewart pointed out, it's awfully strange that that constitution would lead on some examinations to have one film [and on others to have] three films. Those are technical matters related to the x-ray department. And then to have the risk turn out directly proportional to the number of films doesn't seem to argue for constitutionality. But still that's the big thing they're using. It's just two forks: constitution leads to x-rays; constitution leads to leukemic children. Don't want to face that reality.

Well, then they had an ostensible study of children who were in utero during the atomic bombing, looking for the development of cancer or leukemia in the next 20 years. Kato has published and says he doesn't confirm the Stewart finding, that they are finding far too few cancers. That has been latched onto as disproving the Stewart study completely. There have been defenses and answers on that subject.

During the course of writing the book this year, I decided to look at those detailed data from Kato in Japan. I found the most startling revelation, which will be in the book, and it's something Kato doesn't know is in his paper. When it comes out in my book, it'll embarrass the hell out of him. I calculated how many cancers and leukemias should have been expected and I think the total was eight and he found three; eight if Alice

Stewart was right and he found three, so five missing cancer cases. Statistically, five could just be missing in a given sample. Nevertheless I worry about that; I'd like to know.

But there's an amazing thing in there. In one section of his paper he shows the number of people who died of unknown causes versus known causes of death. It turns out the higher the radiation dose of these children, the more of them died of unknown causes. And it was so large an effect that there's one chance in a thousand that I could be wrong on this conclusion. What that says is that his diagnosis is just not worth a damn on those studies and therefore the Kato papers, which are widely touted as refuting Alice Stewart, refute nothing. They may tell us a very interesting story. I think what's going on there is a sociocultural thing. In Japan the fact that you were one of those who were atom-bombed makes you a pariah. Were you aware of that?

Hughes: No, I didn't know that.

Gofman: There's a strong social stigma attached to having been atom-bombed. They're tainted people. No one would want to marry with a person who'd been atom-bombed. They're called hibakusha. There's a strong taint. I rather suspect that must have started early, and quite likely a child [of] a radiated woman, where the radiation dose was high--to say that child had cancer would have doubled the social stigma. And so I suspect that either the kids who were dying of cancer weren't brought in --they just got a death certificate saying "cause unknown"--or somebody actually prevailed on physicians: "Please don't put cancer down...."

That's a common thing in the United States even today--families asking physicians not to list insanity, not to list cancer on a death certificate. But in the case of Kato's paper, I've got him right by his own data showing that as the radiation dose went up, the fraction who had "diagnosis unknown" rose astronomically.

So I believe the Alice Stewart data are correct. I believe they push us down to one-quarter to one-and-a-half rads and the linearity must go all the way to zero. That's my opinion.

The Committee for Nuclear Responsibility

Gofman: I think the Committee for Nuclear Responsibility has done a very good educational job. It was established by Lenore Marshall, a poet who was concerned about nuclear matters. She asked me to serve as chairman, which I did. We've been going since 1971. We have a very distinguished board of directors who support the Committee.

Hughes: How actively?

Gofman: Not too actively. Some of them occasionally write papers for the Committee. Dr. Lewis Mumford has written papers, Paul Ehrlich has written some for the Committee. Most of them are supportive but don't actively write for the Committee. But they've been very helpful, the board of directors.

Mrs. Marshall died very soon after the Committee was established--died of cancer, by the way. She gave the Committee its initial $50,000, and since then we have just relied on public contributions. There have been about eight or ten thousand people who have at one time or another contributed, and some of them continue to contribute. By personal choice, I have never wanted the Committee activities to grow very large; just don't want a big organization. The reason for that is, if you get a big organization you then find yourself needing big funds. If you need big funds, you sooner or later start going to foundations for money and that's the end of your independence.

So I've kept it such that we do a limited amount of work on selected topics of educational value--on the hazards of radiation or the problems of nuclear energy or the economics or availability of uranium. We've worked on a number of different subjects and we've done it all within a budget of about $40,000 a year, all raised from the public.

Hughes: Is it the public that you are aiming at?

Gofman: We're aiming at several places, reasonably successfully. I'd say when we put out a report from the Committee for Nuclear Responsibility, certain highly technical reports will go only to 500 people. But they eventually find their way into places that if we put it out to 500, then maybe 100,000 people see it, because a number of agencies reprint our publications.

We have one policy that I think is rather unique for organizations: on every paper we write, we say please feel free to reprint this without requesting any permission, and people do. Some of the things we've put out to, say, 3,000 of our contributors, I'm sure have been seen and reprinted in numbers like 500,000. We've seen them in other places. A large number of publications that we have put out have been reprinted in the Congressional Record, put in by Senator Gravel or others. So I'd say probably 90 percent of the Committee for Nuclear Responsibility publications have ended up in the Congressional Record earlier or later. So they get to be seen by all kinds of people in government.

We maintain an active press and media list of about 500; they get everything we put out. We probably do as much education of individual reporters, of press and media, as any organization --a constant stream of letters, phone conversations --in addition to the reports we give. So as a small effort, purposely small, I think we have an impact. I think our reputation is very good, that the information we put out is reliable.

Hughes: Would a reporter tend to consult you whenever any sort of an accident or newsworthy item occurs?

Gofman: Yes. As a matter of fact that's one of the difficulties of the position I'm in. That is, that if anything comes up, if a new BEIR controversy comes up, or if there's an accident or a release, we get calls from reporters all over the world. We actually get some from abroad; we've gotten calls from Australia, from Sweden, from England, from Switzerland. Then there are often some very lengthy conversations to try to straighten them out. A number of the media reporters will do taped interviews in their questioning; they want to put it on the air. That's been enormously increased since the Three Mile Island episode. It takes a lot of time. That was one of the reasons why I decided to write the book I wrote this year.

After the Three Mile Island accident, the official government agencies and the nuclear industry mounted a very much increased campaign to downplay the hazards of radiation--after all, it's just like an x-ray, things like this; we didn't get above the safe limit--when the major thrust of all our work and the linear hypothesis is that there is no safe dose. So reporters were constantly calling and seemed to be shocked when I'd say, "Well, there is no safe dose."

"But we have right here the government release stating that this was below the permissible." I [would say], "Permissible has nothing to do with safety." In fact, I even have a letter from the head of the safety standards division of NRC, Robert Minogue, confirming he doesn't believe there's any safe dose either, and that the standards are just a trade-off of how many deaths you're willing to have in exchange for the energy.

I got so discouraged by the fact that reporters [across] the nation were being fed these handouts, seemingly believing them. I figured if 20 or 30 of them call me up, that must mean out there in the field there are 20,000 of them that are confused, because they're not all going to call me. Probably one out of a thousand calls me. And that's when I decided to write the book Radiation and Human Health.

In the introduction of the book I said the real purpose of this book is to demystify science and just to let any intelligent nonprofessional member of the public, with a little application, be in a position to handle this evidence as well as any expert in the field. And to the extent that I wrote the book right, it'll do that. To the extent

I made it too complicated, it won't. We'll have to see.

Hughes: Are the reporters in general interested in the statistical basis of the arguments?

Gofman: Yes. You can't very well on the phone go into the detailed statistical treatment of why somebody is wrong or right on a given thing. But they are interested in knowing: "How did they get that answer? How did they get this statement that nobody was hurt?"

Athena Linos et al.

Just recently, for example, a paper came out from the Mayo Clinic in a very highly respectable journal, The New England Journal of Medicine, by Athena Linos and several other colleagues, which purports to show that radiation in a region they call low dose, from 0 to 300 rads--which I call huge radiation--does not increase the risk of leukemia. Now, I've had a number of reporters call me about that. "What do you have to say about that?" And I say, "Do you have a little time?" And they all have time and I run through the list of ten disastrous errors of that paper.

One is very disturbing to me in particular. I've seen all kinds of scientific papers, but rarely have I seen one where all the conclusions are presented and no data. And that's what this paper is. I said to myself, "How is it that the New England Journal editor would ever permit this to be published?" I've sort of justified that in my mind, that there have been papers showing radiation's very bad, radiation's not so bad, in the New England Journal. Maybe they feel they're just going to make it a forum and if somebody wants to publish this paper on one side, they're just not as critical. But generally you wouldn't expect a paper to appear without the data, so you could look at it and say, well, how did they get this? It's conclusory in toto.

So I wrote a letter to Dr. Linos at the Mayo Clinic and said I read your paper with great interest, but I don't have any tabulations of the data that would allow me to test some of the conclusions you drew. It would be much aprpeciated if you would send me a few printouts--it's not a large number of cases you're dealing with--a few printouts identifying the cases that led to these conclusions. [Dr. Linos did not reply because she never received the letter.] I've treated Dr. Linos's paper in the book in detail.

Hughes: It would be interesting to have a conversation or a letter from the editor of the New England Journal....

Gofman: I think I might like to write the New England Journal editor and say, "How does it happen that the data were not published with the paper?"

Hughes: Well, I would think that particularly in a journal with as much credibility as the New England Journal of Medicine--I draw a parallel with the AEC's saying 0.17 rads is the permissible dose; it has the same credibility. Here's a paper that appears in the New England Journal, it must be good stuff....

Gofman: Absolutely. I would not only say that, I would make the guess that those journalists who picked up on it and say that they've now proved that low dose radiation, even up to 300 rads, doesn't hurt--it's going to hurt, and giving a tremendous fraction of the public the wrong information, and the medical profession... Think about it for a moment. Think about what I said earlier about the doctors--they don't want to think that radiation hurts people because they're giving radiation or advising it. Now, with a credible [publication] like the New England Journal coming out with a paper from a credible institution like the Mayo Clinic saying, hey, look, there's nothing to worry about--I think thousands of doctors will say, "Well, that whole stuff about radiation being harmful is malarkey."

They're not even going to look at the paper, just the fact that somebody says, "Hey, did you see this thing? The Mayo Clinic says leukemia isn't even caused by radiation, even up to 300 rads. Just appeared in the Mayo Clinic." Most doctors are not going to ask, "Did you look to see whether they had the data in the paper?" They're not going to do that at all; they're just going to go by the conclusions. They're not even going to go back and look at the paper; they're just going to say, "Well, somebody in our medical society got up and said the Mayo Clinic has now proved that radiation isn't harmful."

I think it's a disaster the way these things happen, and I don't know of a way to stop it. I think that the editor of the New England Journal is certainly culpable for not having requested that the data be published with the paper. I'm just amazed at that. But if the data were available, then I think that they certainly should publish them. I'm in favor of that, even if the conclusions may be grossly at variance with any I might draw. The worst thing in the world would be any effort on any editor's part to suppress anybody's views, although editors do that.

This paper is flawed on many, many grounds. It's worthless as a scientific paper even if they provided the data because the person who chose the cases for inclusion, the controls and the leukemics, was none other than Athena Linos herself, by her own admission. If you're trying to do a study to see what's the x-ray history of controls versus disease, the last thing you want is any of the investigators having anything to do with choosing the controls and cases. By admission in the paper, she chose them, which makes the paper worthless no matter what the data show.

There's another interesting thing: If you would ask people, "What do you consider low dose radiation?"--I looked up what various other people say. A government committee said anything under one rad; some people think of low dose being 10 millirads--that's a hundredth of a rad. The Linos paper calls 0 to 300 rads low dose radiation. Everybody's criticized for using high dose data when they use 25-rad data. This is 300 rads. I said to myself, there are many peculiar things one can do, but the most peculiar would be to write a paper referring to 300 rads as low dose radiation. There has to be a reason.

So I scanned and scanned the paper and found the reason: They had some data on cases with records of over 300 rads they said were excluded because they wanted to stay only in the low dose region. Well, that's absurd, because 300 rads isn't the low dose region. So I looked at the data above 300 rads and there is a powerful confirmatory case proving the leukemia association with radiation. The chi squared on it was so high that the p value was less than 1 chance in 10,000 of being wrong. So in order to throw these cases out, they declared a definition of 300 rads as low dose radiation. So beyond the errors of having Athena Linos doing the selection of cases, then [there is] this throwing out the people in the 300-rad thing. It looks like a little bit of fraudulent manipulation on top of everything else.

Hughes: Did you mention that in the letter?

Gofman: Her letter? No, I just asked her for data.

In the book I've described it very well. There's an interesting thing: frauds often don't cover their tracks well. People have asked Richard Nixon, "What the hell did you keep those tapes for?" Well, I asked myself, just why in the world did Athena Linos even put in these cases above 300 rads that they said they threw out? They're the only data they did put in, the listing of the cases they threw out, which enabled me to prove that they threw out a powerful story.

I just wonder often whether there's some psychological thing that almost drives them--if they're going to carry through a fraud, they somehow put along with it the evidence that will destroy their fraud. It almost seems like something is driving them to do it. I wondered likewise why in that Kato paper from Japan he put in this evidence, which was gratuitous, about the fact that at the higher doses the diagnoses [were rarely known]. I just wonder why that happens?

Arthur Tamplin

Hughes: Well, changing the subject again, you've mentioned Arthur Tamplin many a time. Would you say something about him as an individual and also what your working relationship with him was and is?

Gofman: Yes. Arthur Tamplin got his Ph.D. in biophysics under my guidance at Berkeley, working on the lipoprotein program. He handled a large part of the operational work on the ultracentrifugal lipoprotein program. I came to respect him very highly.

As a matter of fact, he didn't start out as a graduate student; he applied for a job. We needed somebody to run the centrifuges and he ran them. Very quickly I saw this guy is very bright and so I got him involved in the analysis of ultracentrifuge films and he picked it up in a moment. Then he later enrolled as a graduate student, did good work. Then he went off and he worked with various companies, including a stint at the Rand Corporation and with some large corporations.

When I decided finally to go to Livermore, he was one of three people that I thought would be very good to get to handle one part of the project we had to do, namely pulling together all the world literature on fallout and radiation effects. He did come to Livermore as head of that division and did, I think, a very excellent job in it. I had and have a very high respect for his intellectual ability and his integrity in science. He's just not buyable, I don't think.

The radiation effect was sort of a back-burner project that came up finally in 1969. That wasn't my mainline effort. As I say, this whole question of evaluating hazards was a back-burner project that he and I were doing. In 1969, when I had the invitation to be at the engineers' meetings, I said, "Art, we've got these data in good enough shape. Why don't we present this at this meeting?" So we did. I gave the talk, but it was jointly written.

Hughes: Can you say something about the division of labor in the lab? First of all, would you call it a partnership?

Gofman: With Tamplin?

Hughes: Yes.

Gofman: Yes, I would say it was a partnership. He had done a lot of the background work, pulling a lot of stuff together. I sort of got in somewhat later and got interested in the calculations. Then when we got going further, I probably spent a lot more of the labor than he did. But we both participated and I'd say there were real contributions from both of us.

Hughes: Is he very vocal?

Gofman: As a matter of fact, he gave that talk at the American Association for the Advancement of Science and he gave several other talks. He was already speaking at a time when I had said practically nothing, except for the writing. I remember Nucleonics Week, a McGraw-Hill publication that reports things for the nuclear industry, referred to the two of us as Tamplin being the more vocal one that was out on the hustings speaking, and Gofman seemed to be involved in the writing but rarely spoke--which wasn't quite the way it was. I don't know how they got that impression. But he obviously was quite vocal at that time and still is.

I don't see Art very much at all anymore. We've talked on the phone, maybe the last time about three months ago. In '73, I left the Lab and he stayed on.

We had tried to move all of our work to the Boalt School of Law in 1970, to set up a center in the School of Law for the adversary study of science, to provide the other side of the picture on a number of issues of great public concern. The faculty of the School of Law voted unanimously to embrace this project. I went back and saw the Ford Foundation and they said they would probably support it. [Richard M.] Busbaum, the head of the project at Boalt, backed it. And then Ford just dragged their feet and dragged their feet.

Then about a year and a half into it, I was talking with Busbaum and Ken Phillips, the associate director of the law school--I was still at Livermore; so was Art. We were hoping to move Art and me to a project engaged in trying to provide the scientific basis for considering the other side of the question on a number of important issues. Dick Busbaum said he'd heard some rumblings that maybe the Ford Foundation felt that I was too outspoken. I said, "Why don't we solve that right now by my dropping off the proposal? I don't need to be on it. The important thing is to get the adversary center set up." So they dropped my name and they went on negotiating with Ford for another year and a half with Arthur and Don

Geesaman in it, and it never came to anything. So by '73, that looked dead.

Hughes: You were just too politically hot for them?

Gofman: Yes, for Ford to take on, right. Just too hot. So they didn't. And the idea died at the law school. But they were really enthusiastic to have it over at Berkeley School of Law.

In February '73, I resigned and came back into my teaching full-time at Berkeley. Tamplin stayed on at the Lab. Then he got a year's leave of absence from the Lab; they were happy to see him go. He worked for the Natural Resources Defense Council in Washington. Came back to the Lab and he stayed on I think six or seven months, maybe a year, during which period he said he was treated as a non-person.

I was gone already and he was still writing and doing whatever he wanted. They didn't want to assign him any work. They were just tolerating him because they apparently elected that they never wanted to have on record that they fired him or me. So after six months or a year back, he left. He resigned from the Lab and went to work for Natural Resources Defense Council in Washington as a staff scientist, where he is still--and I think is still effective.

In fact, I just saw in a little publication that Arthur Tamplin and Elizabeth Shaefer--she's the wife of a New York doctor, they're both supporters of the Committee. She wrote me a note saying she had been doing some work to help Arthur Tamplin's project. I saw this little report: Arthur Tamplin and Elizabeth Schaefer have concluded that the BEIR III Committee's estimate is too low by ten times. Their upper limit is too low by ten times. The true number of deaths for one rad given to the population would be 4,500, and the number in my book is 3,771. So we're still on the same wavelength.

Hughes: Yes, I would say so. Can I backtrack again? In that move to Lawrence Livermore, you mentioned that two or three people went along, including Arthur Tamplin.

Gofman: Right. Oh, a number came from Berkeley on the more technical end.

Hughes: You mean specifically from Donner Lab?

Gofman: From Donner, yes, a number came with me. But from the outside, Bernard Shore, who was at Washington University in St. Louis, and his wife, Virgie, a fine biochemist. He had gotten his degree with me. He agreed to come back and head a section of the experimental work.

Russell Bjorkland, who was a former graduate student of mine, came to head theoretical work. Russell is a brilliant theoretical biophysicist who can never bring himself to carry a project to a conclusion. Sad. He's very, very bright. Did some good work at Livermore, showed it to me. I went over it in detail with him, an idea on the folding of proteins. And he never would write it up. Never. It hasn't been written up yet. So he only stayed a few years because he just couldn't get himself together.

Shore stayed on and he replaced me as head of the department and tragically died in January '78 of malignant melanoma. He's one of the cases of malignant melanoma at the Lab.

Hughes: What do you have to say about that?

Gofman: About the malignant melanoma?

Hughes: Yes.

Gofman: I'm not convinced that there really is an excess out there. I'm not convinced that this whole public flap is correct. The reason I'm not convinced, Sally, is that I know from having established the medical department there that everybody at the Livermore Lab gets looked at once every year or two years. And if they have a suspicious lesion, they're going to be told to go to a doctor and have it looked at or biopsied. But the rest of the people in the Livermore Valley community don't have that exam, they're not going to be looked at as much, and there are good reasons for believing that you'll find melanomas more in people who have this periodic medical exam than the others.

The report on the Livermore Lab is only on incidence of melanomas versus the community, and it's three to five times too high. It's just the incidence. If the report shows in the future that deaths from melanoma are two or three times higher at the Lab, then there is no way other than to believe it. But so long as it's incidence, it has the possible error of being due to more diagnosis. I don't know what to say at this time. We'll have to wait for the final evolution of the Livermore melanoma story.

Termination at Lawrence Livermore Laboratory

Hughes: You left Lawrence Livermore in 1973. What was the overriding reason?

Gofman: The overriding reason I left relates to what I was saying about the fact that Roger Batzel and Mike May did some good things as well as bad things in not backing me.

Roger Batzel came to see me in the spring of '72. As I said, we had kept up our conversations all during this strained period. He said, "We've got a problem. The AEC came to us last year"--'71--"and said, 'We've got to phase out your chromosome and cancer program.' And Mike and I argued that we disagree totally with John with what he's doing on the radiation, nuclear energy, health standards issue, but we have a high regard for his chromosome cancer program and we're not going to cancel it." So he said they went away.

But he said they'd just come back now and said, "Cancel Gofman's program and take away his $250,000 of support for that program." That's what I was costing them, with about 12 or 13 people working with me, and computer and engineering time. "Cancel this program, or if you won't do that, which you refused to do last year"--that is, speaking to the Lab management, not to me--"we will just delete $250,000 from the Lab budget."

So Roger came and he said, "You don't need to believe any of this, but this is what we told them last year: We would not cancel. I'm coming to you now to ask you what you want to do about it. If you tell me you're going to stay and keep your program up, we will cut back $250,000 from other parts of the biomed program, just because we think your program's good. But it's going to be your choice."

So I said, "Well, Roger, I'm not going to make the choice to have anyone lose his job because the AEC wants to punish me. Let's see what there are in the way of possible options. Since I'm back teaching half-time now, I'd be happy to move the program into Berkeley if I can get some support." I said, "Could I move the equipment if I do get the money?" We had about nine intricate microscopes set up, some scanners. He said, "Look, we can just arrange that with no trouble. If you can get the support, we'll move the stuff in."

So I called Nat Berlin, a former student of mine who was then an associate director of the National Cancer Institute, and outlined the situation. I said I'd like to come back and talk about it. So he arranged an appointment with Frank Rauscher, who was then the head of the National Cancer Institute. And in eiher March or April of '72, I went in to see Rauscher.

We spoke about two hours. He was most cordial. He said, "Nat has filled me in." I said, "You're aware of all the controversy." He said, "Oh, yes, I'm quite aware of the controversy." And he knew about the chromosome part. He said, "You know, it's a fortunate thing, because this is just the kind of program we think would be needed for the Cancer Institute to support. We have some projects at Yale"--he mentioned one and a couple of others--"where someone to analyze the chromosomes by this technique would be very valuable. Would you consider doing it?"

I said, "Not only would I consider doing it, it's just the sort of thing we'd like to be able to do." He said, "Look, it's going to take me three or four weeks to arrange it. But I think I can get you the funds to move your program into Berkeley." Particularly since there wasn't any requirement for equipment funds; we were going to move it.

I came back and went to see Roger Batzel. I said, "It looks as though I'll be able to get the support." I explained it all to him. So he said, "Fine. Let's wait and see what Rauscher says." It wasn't an urgent request; I had at least several months.

Well, the three or four weeks passed and I didn't hear from Rauscher. So about seven weeks passed and I still hadn't heard and Roger was asking me, did I know. So I just dropped Frank Rauscher a note saying, "Dear Frank, no hurry, but you thought maybe you'd know by now." And there was the most strange thing in my life. I got a letter back from a deputy of Rauscher's: "Thank you very much for your inquiry about support from the National Cancer Institute. At the present time, this area of research is not in the mainline interests of the National Cancer Institute, but if you ever have any ideas in the future, please do not hesitate to propose them."

So I knew what that meant. What happened was, Rauscher must have talked to someone else high in government and they must have... I'm only reconstructing, my guess. After having told me it looked like a sure thing, to get this letter that he wouldn't even answer himself, I concluded that he must have been told by a higher echelon, "Hey, look. This guy's trouble. What do you need to get the National Cancer Institute in trouble for? Just stay away from it." And he just didn't know how to say it to me, so he didn't. He did it in a way that would just cut it off.

So I went to Roger Batzel and said I had failed to get the support from the National Cancer Institute. I showed him the letter I got from this deputy and he seemed to be upset by that, too. So he said, "Well, what do you want to do?" I said, "I don't know of any other place besides the National Cancer Institute that could provide the funds. Let's kill the program." So that week we killed the program. Nobody lost their job because all my people were transferred to other things in the biomed division. And having exacted their retribution, the AEC forgot all about taking the $250,000 away. From the AEC point of view, everybody was happy. My program was dead; the people kept their jobs.

So I said to Roger, "I like the Lab as a place to do research, but since I no longer have any research program, there's not much point in my staying in the Lab. So I think I would like to return to full-time teaching." You see, I had that agreement--I could go at any time. I said I'd like about six or nine months to complete some writing. He said fine. And then he said, "By the way, you commute out here to Livermore. For the last six months, if we could find you some space at Berkeley on the Hill until you move back into Donner and you're teaching, we could save you the travel." So I said that was fine but I wanted my secretary and one assistant. He said, "Oh, that's fine. We'll pay their salaries, too, until you resign."

So I did move back into Berkeley, on the Hill, Building 90, and was given a suite of offices. Margaret Soderberg, my secretary, and Erma Kovich, a gal who had worked with me, came to help me. I did the writing and then, in February '73, I officially resigned and shifted to my full-time teaching. So [those were] the circumstances in which I left.

Association with the Medical School

Hughes: What has been your association with the medical school?

Gofman: Recently, very little. I go to rounds occasionally. The first two years that I was an instructor in the medical school as well as being at Donner, I actually worked in Professor Faulkner's clinic and taught senior medical students and did some research with them. I came over two days a week.

I told you about the flap with Dr. Kerr about the heart disease work. But then during that period and thereafter I've had some collaborative projects with a number of people in the medical school during the heart disease work. As I said, Alex Simon and Felix Kolb, Nathan Malamud, several people in the department, I did research with. But after those first years, when I got busy on the lipoprotein program, I never went back to teach at the medical school. So I've been listed as a name.

But I was really honored in the early 50s--perhaps the most important thing to me about my association with medical school. There was a Professor Leroy Briggs, who taught us clinical medicine at the county hospital [San Francisco General] the junior year. The junior year, the lecturers are mostly not full-time people. At that time, Briggs was practicing downtown, but he was the chief of medicine at the county hospital.

Everybody just had enormous respect for Leroy Briggs. Nobody would ever miss a lecture of his, and that was very different for other lecturers.

For two years running, in '52 and '53 or '53 and '54, I can't remember which, Leroy Briggs called me and said would I give the lectures in his course on bedside medicine and coronary heart disease. He said, "You know a hell of a lot more about this than I do." That wasn't true, but he was intrigued by the research. So for two years I gave the lectures on coronary heart disease in his course, which I consider a greater honor than almost any I think I've ever had, to have him think enough to want me to give those lectures.

But that about was it. I think I participated in rounds on heart disease once or twice thereafter. But my affiliation's been very little. They never removed my name from the department.

Hughes: But no repercussions from the AEC...?

Gofman: Oh, no, I've never heard a thing about it, nothing. I don't know Holly Smith very well, the new chairman. I have a lot of respect for him. I like his rounds. When we did the chromosome work, I sent him over copies of the paper and he acknowledged that they were interesting. That's all.

Early Retirement

Hughes: Why did you decide to retire early?

Gofman: Why I decided to retire early is a little bit difficult to answer. I liked the teaching, but I was caught up in the '73, '74, '75 period with all kinds of requests to go here and go there and talk and do things, and it was a conflict. If I'm teaching, I do not go away on trips. I just don't turn classes over to people--as many do, by the way. So I sort of felt in a peculiar situation. I have all these requests to go lecture, do useful things, talking, and I have these classes I'm teaching, and I can't get any research funds. I said, what the hell's the sense of this? I can take an emeritus status. I'll get a lot less salary, but I'll have all my time to myself. As an emeritus professor, if I can get a grant, I can always come back and do research.

By the way, two years ago I reactivated my research status so that if I do get a grant I can legally move right back in. And if I ever want to teach a course awfully badly, there's no problem getting permission to teach. In fact, I'm considering doing that once the book is available--giving a course or seminar on radiation and human health. I had set up some graduate seminar courses on atherosclerosis for one quarter, and carcinogenesis the next quarter. I did that for several years. So I said, it'll give me all this flexibility. So I decided to take an early retirement.

If you'd ask me, versus being in the lab doing research, would I do [anything other than] what I'm doing now? No. I would be in the lab doing research, versus just teaching and not having any research. I can do just as well with the flexibility. I'm doing a teaching job in a different style now. So I find it's got its compensations and its losses--the losses being not having a lab program like the chromosome program, which I enjoyed doing. The compensation is that I've been able to do effective work. I've done that reanalysis of the Hanford workers. You've seen that in Health Physics?

Hughes: Yes, I did see that. Should you get support for your research, would the Donner Laboratory be the place you would go?

Gofman: I think so. I think I'm still welcome there. Certainly Mortimer and Alex Nichols have been enthusiastic about my coming back. That's the place I have an official affiliation, my emeritus status. I don't think they necessarily have to give me space in the Donner Lab, but the university does maintain the idea that an emeritus professor will have space to do research if he wants it. I might like to be back in the Donner Lab, to do the work. I don't have any bad feelings about the Donner Lab per se.

Institutional Requirements for the Use of Radiation

Hughes: In 1947 Ernest Lawrence appointed a committee headed by Earl Miller, the radiologist from the medical school--and there were other members: yourself, Hamilton, Lawrence, Stone, Alvarez, and Tobias--which was to oversee the safety aspects of the use of radioisotopes and radiation in the Rad Lab in general. Do you remember the circumstances behind the formation of that committee?

Gofman: No, honestly, Sally, I don't remember the cirumstances. I do know this, that Ernest was extremely concerned about health and safety practices at the Lab and just really didn't want to tolerate anybody who wouldn't conform. Possibly there had been some incident or some statement by someone that might have spurred him into action. But I can tell you something about that committee: I certainly don't remember it ever doing anything. We very possibly met once, but I don't even remember that.

Hughes: You don't think it was anything that the AEC said should be done in all the national laboratories?

Gofman: It is not inconceivable, but I don't remember. What year was that?

Hughes: 1947.

Gofman: Just after I came here.

Hughes: Yes, exactly. I was assuming you were appointed to the committee because of your medical background.

Gofman: Yes, well--no, I just don't have a recollection of that.

Hughes: The very fact that you don't have much memory of it certainly substantiates the thought that it wasn't very active.

Gofman: That's my recollection.

Hughes: I know Miller did spend the war over in Berkeley, though. Exactly what he was doing all that time I don't know.

Gofman: I don't know what Earl was into, but he wasn't in any way affiliated with anything I was involved in, the plutonium work. He might have worked some with Hamilton.

Hughes: Yes, he did. I have the understanding that his appointment was really in connection with safety, but he may have been doing research as well.

Gofman: I never saw him there. He could have been doing research and he might have been involved in safety. He was, I think, sort of a protege of Robert Stone, who was named the head of the medical department of the Manhattan Project. I don't remember seeing Earl during the years before that. I later came to know him, but not before.

Hughes: Another question about human use committees or their counterpart: Do you have a recollection of when that whole system was set up?

Gofman: No, but I think I remember there was talk about it in the early 50s. And I think R. Lowry Dobson probably was involved in it and knows about it. Dobson is now at the Livermore Lab. He was medical director of the Lawrence Lab in the very early 50s and he was working in Donner Laboratory. He has a brother, Ernest Dobson, who also worked in Donner Lab. Lowry, who's a physician, worked for a Ph.D. at Donner and was the medical director at Lawrence Lab. Then he went off in the late 50s to work with the United Nations in Geneva. And then I brought him back to the Livermore Lab and he joined the biomed division. He's there now. So he would know. I'm sure he was involved in the human use committee; I don't think I was. But I remember it was in the 50s.

Hughes: What I'm trying to get at through all these questions is when concerns became great enough to formalize an institutional structure that would deal with this whole issue of safety, rather than just one individual, in this case Ernest Lawrence, having that concern.

Gofman: I suspect it just sort of grew in the 50s.

Hughes: Was that because of increasing knowledge about the potential danger?

Gofman: Increasing use. I can't recall whether either the Commission or individual institutions may have begun to see the beginning of any lawsuits or criticisms. Or that sort of thing could even have come about from some of the legal people or the university administration people saying, hey, this is going to get us in trouble. That'd be one of their jobs all the time, to look for such things.

So it could have even come from liability considerations. I don't know. Certainly increased use. And it was very loose; everybody did what they wanted to do. I just know that nobody checked me out or anything like that about going into the Donner Lab clinic and administering radioactive phosphorus to people and some materials that I made myself, actually. So it was very loose.

Hughes: You just did it.

Gofman: You did it, yes.

Hughes: Do you remember when badges came into use?

Gofman: I think there were badges by 1950 for sure. In the war years around the cyclotron there were some of these fountain-pen ionization chambers, but there were no badges. But I think by 1950 the Rad Lab had badges.

Growth of Nuclear Medicine as a Specialty

Hughes: What about nuclear medicine itself? Do you have any feel for when that became recognized as a field unto itself--particularly recognized in the sense that it was included in the medical curriculum, at least in some medical schools?

Gofman: Probably in the 60s. Actually, nuclear medicine was what people like John Lawrence and Joe Hamilton were doing. That was nuclear medicine, the beginning of it. There were a number of people from other institutions who came and worked with them, went back to their own institutions. I'd say early 60s probably.

Probably the best way to get a good idea would be to see when the first issue of this Journal of the Society of Nuclear Medicine was published; I don't really know. It's pretty well institutionally established by now. They've put out major sets of medical reports on the internal radiation dose from a variety of nuclides used in nuclear medicine. It just steadily grew, is my recollection. Certainly by the early 60s it was far along as an entity.

Hughes: Do you have anything to say about the role of the Donner Laboratory, even the Division of Medical Physics, in the whole discipline of not only nuclear medicine but medical physics in general?

Gofman: I think the Donner Lab spawned an awful lot of that whole discipline. I'm not an enthusiast for nuclear medicine. I feel it's being misused, much too much being used, probably causing a lot of future cancers for not a good benefit. I think Donner's partly responsible for that overuse because they've been enthusiasts for it.

A lot of the people got trained there and they're now heading departments of nuclear medicine elsewhere. It's been the cradle of a lot of the nuclear medicine, and an awful lot of the people in medical physics around the country got started at Donner. So it's an important laboratory in that sense. But I'm not an enthusiast for nuclear medicine.

Hughes: Were you aware of any of the frictions between specialties? I'm thinking particularly about the radiologists, who apparently considered nuclear medicine their prerogative.

Gofman: Yes, I remember that early. I think there were a lot of concerns of that. Robert Stone I believe was quite jealous of the existence of John Lawrence's clinic in Donner in a variety of ways. I don't remember details now. It was quite evident that he was. The argument was often that really it belonged in a radiology department with experts in that field.

Hughes: Were they training radiologists in nuclear medicine, or whatever they called it then?

Gofman: It would be just casual. If a radiologist happened to go to some center that was using radioiodine or radioactive phosphorus, he was trained. Some radiologists I know did come to Donner and spend a half-year or a year. But it turns out, looking back in retrospect from what I have come to know, the very last people in the world who deserve to have anything to do with anything involving radioactivity because of their superior knowlege of health issues would be the radiologists.

Use of Radioisotopes and Radiation by the Medical Profession

Hughes: Has that ever been part of your campaign--the medical use of excess amounts of radiation?

Gofman: Yes, I've said quite a bit about it. I've written a little bit about it, a little more than a little. I wrote a paper in '74 or '75 [that] said I thought that the medical excesses in the use of radiation were responsible for somewhere between 12,000 and 40,000 deaths a year.

Hughes: Is it the number of x-rays that disturbs you or is it the intensity?

Gofman: It is both. The situation is, number and intensity come together, simply because it is the total accumulated dose that matters. So if you have a very large number and a modest dose, it's as bad as one procedure with a fairly high dose. In the book I've just completed on radiation and human health, I have an extensive chapter on the medical uses and misuses of both x-ray and nuclear medicine. I'm really rather negative on both, but negative in what I consider a sensible way.

Let's take a child with a congenital heart lesion of some sort and let's propose that angiography be done, which can mean a lot of chest films. The dose is quite high in some of those procedures, at the most sensitive time of life. Yet some of the children have a vascular or heart lesion that is not going to be compatible with much further growth and development. They're going to be invalided and die of heart failure or something.

So I think what ought to be done in situations like that, and that's only an illustrative one, is to explain to the family as carefully and as openly as possible: "Look, this is about the dose that will be given. This is the kind of evidence we have on what that translates into [in terms of] later cancer and leukemia. But here's this disorder, which has a one in three chance of killing this child within three years." As I see it, the family would almost always elect to give the child a chance to live till twenty, maybe die of cancer then, instead of living until three. It's that kind of thing.

It's never, absolutely never, in my opinion, appropriate to say that we can use radiation to help one person's life and dump radioactivity into the sewer system--to say, "Well, the number of people we've saved exceeds those we've killed." You don't have any right to hurt one person to help another. But you do have, I think, the right to give someone an option: "This may hurt you later, but we're doing it because it may help you with something important now." So on that basis I don't object to it.

Hughes: Would you extend the same argument to the heavy-particle therapy?

Gofman: To all of them.

Hughes: Only if it's a life-threatening situation?

Gofman: Absolutely. It's got to be very close to life-threatening indeed.

Hughes: Because those are tremendous amounts of radiation....

Gofman: Right. Now, the same thing is true in nuclear medicine and the reason I wrote so much--I've spoken a lot about it, too--to medical audiences. Recently, '79, '80, there [have been] some articles concerning the use of radioactive iodine for the treatment of hyperthryoidism in children. The purpose is to destroy the function of the thyroid gland. There are papers by Freitas and by Beierwaltes stating--I can actually quote it--"The theoretical risks of such therapy have not materialized." Therefore they're recommending a broadside use of radioiodine in these children.

I've gone through their papers and analyzed them in the book and showed that for the number of patients they studied and the short time they observed them, the theoretical risk was a 200th of a cancer. You can't observe 200ths of a cancer. It's only because they're in the early phase after irradiation. For leukemia, it starts to show up three years after radiation. For most of the cancers, it usually takes ten. So if you do a nine-year study and you don't find anything, you have to be a world's first-class idiot to write a paper stating the theoretical risks haven't materialized. The theoretical risk at nine years is near zero, and it materialized.

Well, I've calculated out that in Freitas's series, the way he is using the radioiodine, 5 percent of his children are going to die prematurely of cancer somewhere in the body from the radiation. A subset of his children, about a fourth of them, whom he had to treat twice because the first treatment didn't take care of it, are a group out of which 10 percent are going to die of cancer from radiation.

Now I do not say that he shouldn't use the radioiodine. There again, you have to ask yourself, if I don't treat the hyperthyroidism, what's the risk of death? I think it's serious. Second, if I treat it with surgery, what's the risk of death? That's the usual treatment. Third, if I treat it with antithyroid drug, what's the risk of death? You have to weigh all these against the risk of cancer. If it should turn out that the surgical risk is one out of five or one out of ten, I think the radioiodine would look better.

I don't think the surgical risk is that high, but it's a touchy question. Surgery for hyperthyroidism is not like an appendectomy, because the patient himself or herself is in a very touchy state and they can go into what's called a thyroid storm, the release of hormone into the system as a result of surgery. So it's not an unmixed situation; but I think these blanket recommendations for the use of radioactivity, like the iodine in young children, need to be made after the most serious medical deliberation. I haven't seen anyone pull together the pluses and minuses of one versus the other.

Hughes: Does a well-educated physican have enough statistics to be aware of the implications?

Gofman: No, they don't. Some centers are getting better, but physicians are not sharp on how to pick out a good study from a bad study. I think I mentioned to you recently a bad study from the Mayo Clinic on radiation. I think the average physician will take one of two attitudes: The Mayo Clinic publishes a report saying radiation doesn't hurt. Fine. Freitas reports a paper saying you can use radioiodine in children. Fine. We use it. Go ahead. That's one set of physicians, probably the bulk of them.

The other group, having heard so often that a study was later proved to be untrue by careful statistical analysis or the discovery of bias, may say, "Oh, hell, I don't believe any of these studies." That's the other attitude: "You can prove anything by statistics. I haven't seen it in my practice."

The statement that you can prove anything by statistics is a stupid statement. It'd be like saying you can prove anything by arithmetic or algebra. Not if you use it correctly--you can't prove two and two is six by arithmetic if you know how to use it, nor will you prove anything false by statistics if you use them properly.

Then they say, "I haven't seen it in my practice," which is of course far more stupid because they don't conceivably have the numbers to see it. Medicine is primitive on the side effects of its own treatment, learning statistically and epidemiologically how you would evaluate those side effects. The situation's a mess.

Hughes: And then you have the blinker effect, too, where it's much easier for the individual in most cases just not to know. There's a heavy psychological overtone, I would say.

Gofman: Oh, absolutely, tremendous. I was called in as an expert consultant on a case--to give you an illustration.

There was a lady who was 21, who had hyperthyroidism. She had given birth to a baby about four months before and they decided to give her a dose of radioiodine. So she went in with her baby to the radiologist--this was a radiologist giving it, by the way. He asked her her name; he handed her this cup and said drink it. On a Friday.

She was nursing the baby. Sunday night it occurred to her, "Hey, can any of this medicine I've gotten get through the milk to the baby?" She had an uncle who was a pediatrician in New York and she called him from Wisconsin, where she was. He said, "Oh, my God, stop the nursing immediately."

They took the child in and the child had absorbed a millicurie of radioiodine, a devastating dose. My calculations were that the future of this child is highly compromised, something like an 80 to 90 percent chance of developing a cancer. See, it isn't the iodine that gets to the thyroid that worries me so much. A lot of that iodine circulates in the body for a long time and for every millicurie you get about seven-tenths of a rad. Some of these treatments, 20 millicuries--that's 14 rads, a huge dose.

Hughes: And a baby, on top of it.

Gofman: That's the worst time of all. The insurance company settled, but that isn't going to help that baby, monetary settlement. When the man's deposition was taken, the physician said, "She had the right name, Mrs. K." And they said, "Didn't you ask her, with a baby in her lap, whether she might be nursing?" He said, "I thought she was a babysitter."

Well, that's very blatant and shocking to you, but I don't suspect that this case was an isolated incident. I don't think they bother to find out anything about the people they're doing. The physician referred her over to the radiologist for a treatment with radioiodine, and she got it.

Something that I learned in the very early 60s: Bill Nolan, a health physicist in the Lawrence Lab, had been doing a lot with medical x-rays and dental x-rays and trying to get them to use their equipment better. There was a society of orthodontists who were meeting--could have been 1961, maybe--and he asked me if I'd give them a talk on the health effects of radiation, so I did. An hour talk and a dinner meeting.

After I got through, one of the orthodontists said to me, "It was interesting, Dr. Gofman, but useless." I said, "Well, that's an interesting comment. Tell me why." This was in the question-and-answer session. He said, "Let me tell you something. I practice orthodontics, and you have to figure that any patient you have is a potential malpractice suit. If I walk into a courtroom with that patient and I haven't taken every x-ray I can think of, the lawyer on the opposite side is going to say, 'You haven't taken this film?' and raise his eyebrow, and the juries will invariably regard that as what's called a res ipso loquitur case--the thing speaks for itself: obvious malpractice for failure to take x-rays."

So, he says, every orthodontist takes everything he can think of, not that he needs them for the treatment, but because he needs them to protect himself. What that means, Sally, is there are tens of thousands of people who are lowered in a coffin fifteen years early in their life on the average because somebody's protecting himself against malpractice.

Hughes: It gets to be such an enormous problem, though, because you're not only dealing with dentistry and medicine, you're dealing with the public that sits on the jury, you're dealing with the lawyers. It's all of society that has to be educated.

Gofman: I've written a lot about that in Radiation and Human Health, the new book. Nobody sues for taking too many x-rays. I recommend it in the book. I thought that was coming or ought to come--really say, "Why did you take all those x-rays of me?"

Now there's another misuse of radiation, but it's not quite only medicine. If you're injured--say you have a leg injury or a back injury in an automobile accident, and you file a claim. Your insurance company is going to demand its x-rays. The physician you go to will say, "I want my own set of x-rays." Their insurance company is going to demand x-rays, and pretty soon their insurance company, if it looks like there's going to be a big claim, wants you to see two specialists of their choice to evaluate the claim. Rarely will they accept someone else's x-rays. "No, I have to have Dr. So-and-so do these x-rays because he does them better."

Some of the people in a suit of that sort can get a dozen sets of x-rays. Then if they want to be sure wheher the thing is healing or not, for ascertaining the size of the claim, there will be serial x-rays, three months, six months, a year. A lot of people who had an injury in an automobile accident have gotten caught up in the medical-legal thing. They don't know it, but their future life was far more compromised by the x-rays they got than from the injury.

[pause in tape]

There are about 2 or 3 percent of thyroid cancers, called anaplastic cancers, that are very malignant. People are dead within a year or two and there's nothing you can do for them with radioiodine or anything else. But the vast majority of so-called papillary or folliculo-papillary thyroid cancers are a very benign disease. I'm not at all sure it deserves the definition of cancer.

I've written on this subject, as have others, that you can't define cancer with a microscopic section. Cancer is what cancer does, and the microscope has fooled people many times. There are certain features under the microscope, like the migration of the lesion into the nodes of the neck, which are suggestive. But I can tell you something: The fact that there are nodes in the neck from a thyroid lesion does not alter the prognosis at all. [It] has spread and yet those people don't fare any worse than people who have a lesion here. Both just don't have a high incidence of death from their cancer, particularly those that are under 40 years of age. There seems to be a higher frequency of some complications above 40. So only a small proportion of what we call thyroid cancer is very serious.

This anecdote I think is important. A close friend of mine in high school, who is a radiologist --I don't see him too often these years, but about two years ago he was here for a visit. At dinner he was relating that his 26-year-old daughter had turned out to be one of those, and he used the term, "at risk." Why, I knew all about the story of thyroid disease from radiation, but I didn't know what he meant by this term "at risk." And I said, "What do you mean?"

So he said, "Well, when she was about 6 or 7 she had enlarged tonsils, so I thought she ought to have radiation to the tonsils." That was very common practice up to 15 years ago. He said that the last year she had had a thyroid cancer removed and they were concerned there might be some residue of malignant tissue in the neck. So they did an uptake and found there was some residual tissue and they gave her 30 millicuries of radioiodine to destroy it. There's a good chance you will destroy the thyroid tissue, but you will also give somewhere in the order of 10 to 1 rads to the whole body.

From my estimates, say 10 rads for a woman of 27, that's approximately a 5 percent chance of a fatal cancer somewhere in her body sometime in the future. But if you look at the risk that any residual thyroid cancer in her neck would have hurt her, it's practically zero. So you exchange a non-risk situation for a high-risk situation by misuse of therapy. Her father is a radiologist, went to a leading medical center, and that's what they did there.

[pause in tape]

I feel that many of the physicians who have been involved in radioisotope use and therapy in nuclear medicine, particularly those supported by the AEC and ERDA and the Department of Energy, where they have looked to nuclear medicine to be the shining light in some otherwise unsavory things as far as the public is concerned--these physicians and scientists just have a psychological block that inhibits their finding out some of the hazardous features.

In my own personal experience, having been associated with John Lawrence and having worked in his clinic and administered radiophosphorus to many patients in the period from 1947 to about 1951 or 1952, I know that John was terribly sensitive to the possibility that what he was doing as his life's work and his big contribution to medicine might be harming people.

He just simply refused to look at that issue realistically. John's early contribution in therapeutic nuclear medicine was the use of radioactive phosphorus in the disease known as polycythemia vera, which is a disorder of excessive red cell produciton. And he came under criticism from some people who suggested that some of the polycythemia vera cases were becoming full-blown leukemias because of his use of P-32. A number of places will not use the P-32 therapy for that reason. But John was intent on proving that this was spontaneous transformation, and indeed many polycythemia vera cases do become leukemias, even treated by non-radiation methods.

But I think it remains an open question whether he has accelerated that transformation. I just observed this in him: He simply could not believe radioisotopes could hurt people. That's just a failing of humans, whether they're physicians or not; they cannot easily face that they're doing harm. It's very difficult to accept. It's difficult for anyone to accept. And I think that creates one of the most grave hazards in science and medicine that I know about.

Radiation has been so intimately tied up with medical physics developments. All the people who are working with it, both physicians who are radiologists and scientists working as physicians in nuclear medicine, simply are going to look for every explanation of what the results are due to other than that they caused adverse results.

I don't know how you solve it exactly, but you've got to solve it in some way that simply is out of their hands, and I don't know how you get it out of their hands. For example, if you were to ask me today: In these leading institutions of nuclear medicine, how many of them have really tried to find out by the most careful studies how many of their patients with a given diagnostic test or a given therapeutic trial have been hurt by it? I'll bet if you looked into it, you'd find there is almost universally a desire not to set up such studies, not to find out.

One of the classics, and it's written about proudly, is the use of radioiodine in adult hyperthyroidism. A cooperative study was done, many centers, to see whether cancer had resulted. It was published in the Journal of Clinical Endocrinology and Metabolism, I think in 1968. Well, the study is just a cruel joke. Here is a group of several major medical centers getting together to study an ostensibly serious question: Have we harmed anyone in the treatment with radioiodine? And the average follow-up time of the study was nine years, which is just before any appreciable number of cancers will occur.

Well, here's a group of serious medical people knowing, or who should know from the literature, that it takes about ten years before the curve starts to deviate at all, publishing a paper saying that we are not seeing any of the theoretical risks materializing.

The study has not been extended. I have not seen any further follow-up of this since that 1968 or 1969 paper. The study may have been completed in 1969, the publication a few years later--I'm not sure. But at any rate, it hasn't been followed further. I haven't seen a study with a mean follow-up period of more than nine years. There's an enormous tendency not to do the study because those who are involved do not want to know harmful side effects. They could even sit here and say to you, "Well, of course I want to know." But don't watch what they say; watch what they've done.

Hughes: Do you think there's any hope from things that seem to be happening recently--not so much a finger being pointed at harmful studies, but just useless studies? I'm thinking now of the controversy over the Pap smear and the fact that it seems to be fairly well decided that having a Pap smear every year is next to useless; the same with a physical for healthy people. It hasn't produced anything for the patient. Of course it produces something for the physician, namely money.

Gofman: Yes, money. And it's produced harm in some cases.

Hughes: But [this discussion] at least seems to me to be a step in the right direction.

Gofman: Yes, those things help. By the way, even the harmful things will in time come out, because somebody finally gets uneasy and does a study. But don't count on it, is what I'm saying. I look at most of those questions this way, such as you were saying about the uselessness of a therapy. I was giving a graduate seminar course in Berkeley in 1973 on carcinogenesis. I think I mentioned to you, every alternate quarter I gave atherosclerosis or carcinogenesis and tried to go over some leading topics in the field. I rather enjoyed that. It was hard work to prepare for, but very stimulating.

I remember one graduate student, a young lady in biophysics, kept talking about the Pap smear. It was in the carcinogenesis course. I said, "I'm not aware of any evidence whatsoever that the Pap smear has contributed anything to anything other than interesting slides." And she said, "Well, that's absurd. That's absurd. I've had them and I've been told that they have saved a lot of women from carcinoma of the cervix deaths."

I said, "Well, why don't you go find the evidence and report it back to our seminar course? I've looked. I haven't looked exhaustively, but I haven't seen it. I've seen the nice brochures from the American Cancer Society, or statements, but I'm talking about scientific evidence." Well, that was just fine. She was going to make that part of her quarter project in the course. I had them do a paper instead of an exam; it was more appropriate.

Well, she found nothing in the literature that was very convincing. So I said, "Why don't you go over to the Cancer Society headquarters--they're the ones who are promoting it--and see what they can produce?" All they produced was antagonism--the idea of this young lady coming in and questioning! That opened her eyes a lot, when she got this antagonistic reaction from the Cancer Society. I looked at it again about two years ago and didn't see anything further. There seem to be some studies in Canada now trying to evaluate nationally what the results are.

Hughes: Why is it that cervical cancer isn't picked up that way?

Gofman: Why isn't it picked up by the Pap smear? I think it is, but I think the problem is that of the many lesions that they say are abnormal and that women are going in to have treated--they use cryoscopic treatment, freezing, [and] some are surgically removed, conization... I don't know if any of them are getting radiation; I hope not. I think they're treating a large number of non-cancers and I think that's the problem.

In theory, from what we understand about cancer, it should be true that there is some stage that's early, and that if you can get to it early and really totally eradicate it, you must have done some good, if it was one that was going to spread.

In practice that's worked out very poorly, which almost suggests that by the time you have even a ghost of a chance of seeing the evidence of it, it must have spread already. That's the only way I can understand why these various studies have not been able to show anything so positive about the Pap smear.

As you know, Hardin Jones was a great skeptic about whether anything had ever been accomplished in breast cancer therapy, and I think that's still a very open question. In theory, if you pick up a lesion early in the breast, before it has spread, and supposedly it's circumscribed, you should be able to do something. It just doesn't work too well, suggesting an early spread--unless there's something we don't understand about cancer in general.

Hughes: This is still in the very early stages, but one of the efforts of Tobias's group is developing this heavy ion radiography, which supposedly will be much more discerning of any changes in density--the hope being, of course, that a tumor or any abnormality can be picked up much sooner than it could by the older methods.

Gofman: Really? No, I'm not following that. May be very interesting.

Hughes: But again, that uses radiation.

Gofman: Well, you have to always balance for the individual. I think that the balance has been poor thus far in medicine. What's worse, when I started to do Radiation and Human Health last year, I thought, as I was contemplating the various chapters, one thing I'll do is certainly have a nice chart or table in there--and I know I can find them in the literature--of what dose you'll get for each procedure, like if you have a gall bladder series or a GI series. I was planning to put this in; I was actually getting the source material.

In the course of it, I came across a paper by Kenneth Taylor in the Journal of the Canadian Association of Radiologists. Taylor decided in 1978, I think it was, or 1979--just to place the timing, which is important--to look at thirty centers of radiology in the city of Toronto, Canada. Now Toronto wouldn't be listed as forefront medicine type, but Toronto would be a city like Chicago or Philadelphia or San Francisco or New York or London; you'd regard it as in the advanced area of medicine. It's not rural India.

He measured the doses received by people--I have the paper here--for a variety of procedures in one center versus another. They are so horrifying as to be unbelievable. In commenting on the fact that in one of the procedures, I think it was the upper GI series, you could get as much as 50 rads--not millirads; 50 rads, that's 50,000 millirads--he said this is a procedure commonly done in young men and women who have no evident disease. Well, in another center that same procedure might give 3 rads or less.

The variation he found was a factor of 30 from one center to another, and that convinced me. I'm not going to put a table in that book saying you're going to get this from this procedure, because people would believe it. But I did put his story in and said, you better take whatever dose they tell you you're getting and multiply it by 10, and then say, "Do I want this procedure?"--because you might just get [that dose]. Unless that radiology office is prepared to start giving you dose and evidence of dose that some independent organization can check, you don't know what the hell you're getting.

In Taylor's paper, in one of the centers, why was the dose so high? Well, it turned out that the regulating switch, to regulate the actual rate of delivery of radiation in fluoroscopy, was in a position such that when they closed the top of the instrument--this is not believable, but true, in a journal of radiology, in 1979--they had to squeeze it down to close the top. [This] bent the contacts out of position and shorted them in the full position all the time so there was no way of varying the amount. The patient always got the full amount.

Now this is not 1909 or 1939, it's 1979. As I say, it's not rural India, it's in one of the best centers. Other papers show that there's a variation on the order of 50-fold with what you can get with a given procedure. Well, that's today's radiology. Think of the way people are being victimized. To paraphrase Taylor, the radiologists didn't have the foggiest notion what dose they were giving. When Taylor and his associates fixed their machines to cut the dose down, the radiologists never knew the difference, never complained that things were any worse or anything. So it's not good.

Atomic Bomb Tests

Hughes: Well, going back to another not very pretty issue, I was wondering about involvement of Donner Lab personnel in any sense in any of the atom bomb tests during the 50s in Nevada.

Gofman: I think they may have been involved in the very early period, maybe in '46 or '47, in some of those tests called Operation Able and Baker.

Hughes: Yes, that I know. [Kenneth] Scott went out there.

Gofman: Didn't John Lawrence go to one of those?

Hughes: Yes.

Gofman: I thought so.

Hughes: What triggered this question was seeing that Jacob film on television about the troops that were placed a mile from the site of the explosion in Nevada. It seems rather logical that somebody like John Lawrence would be consulted about the biological hazard. But you have no recollection of that?

Gofman: I honestly don't remember whether he was or wasn't. I certainly don't remember getting involved in it myself.

Hughes: Well, that was before your real concern arose.

Gofman: Right. I was going to say that it might be unfair for me to evaluate whether Donner was involved, because by the early 50s I was so deep into [the heart disease work] that I really wasn't looking at the whole radiation thing. I really don't think any Donner people were involved in setting that up.

Hughes: In any sense? Even in a looser sense, perhaps advising?

Gofman: I couldn't be sure of that. I don't remember. In one sense I guess you could say all of the labs would be involved. There was that custom I mentioned to you of all the labs supported by AEC--the directors got together once every three months at one or another of the 18 or 20 labs and they would rotate, come to Donner. Shields Warren, who headed biology and medicine, and then John Totter, between them Charles Dunham and John Bugher, they would always be present at those meetings on what's new at the Washington scene of AEC. So I really can't believe that they wouldn't have presented at one of those meetings that we're doing these studies at the Nevada test site. So that at least whoever went, John Lawrence or Hardin Jones, would have heard about it.

Hughes: Surely the evidence was strong enough then. We're not talking about low dose when you're a mile away from a nuclear explosion....

Gofman: Well, the Defense Nuclear Agency, Sally, is still claiming that the dose was low. In fact, I saw in connection with a record of some veteran--I actually filed a report on him, Orville Kelly, who just died recently; he had a lymphosarcoma--finally the V.A., after two reports from me, consented to call his cancer radiation induced. But most of them, they're denying it.

I remember one of them who was in one of these shots where he was very, very close to ground zero, and they actually moved in from where they were when the bomb went off, to show that soldiers could be active right after. The Defense Nuclear Agency says from the monitors they had they don't see how he could have gotten more than 9/10ths of a rad. So the argument could have been, not whether it's low dose or high, but that they were going to give low doses, and that they were not admitting that they were going to give high doses.

Now, to tell you the truth, I honestly do not know at this time whether those who planned those tests with the soldiers knew, not what the harm per unit dose is, but that the men were going to get higher doses, and lied or overlooked it or didn't put monitoring instruments in or were so confused about what the doses would be or had misinformation. I think for sure they were very, very primitive in their appreciation of the fact that it's not just the dose from the bomb that goes off, but inhalation and ingestion of radionuclides. I think they were totally primitive on that and probably didn't even factor it into their calculations.

Hughes: Certainly the Utah business later would back that up--the fact that there was no concern about children drinking milk...

Gofman: Right, right, right. It was even worse in Utah later. There was that episode when the milk got very high in radioiodine, [and] the Federal Radiation Council suddenly found that the safe level of radioiodine was three times higher, with no data.

Hughes: It just boggles the mind.

Gofman: Yes, that whole era was pretty bad. But I don't honestly know, when they put those soldiers in, whether they realized the dose that they were getting. The records are very spotty. You can't find records. They've concocted this story--I say concocted a story; perhaps it's true, but one doesn't get a great deal of confidence from it--that almost all the records of the all the people involved in Nevada or in the Pacific test area were stored in St. Louis and the records warehouse burned. That's been the usual answer in every case that's now come up for concern. There are no records.

It's come to this situation: Every time a case comes up, it is the obligation of the widow or the individual who's suffering from cancer to produce the evidence that he got this from that exposure, not the government's [to prove] that he didn't. They just say, "We don't have a record."

A very famous producer in this country--I was talking with him on the phone just two weeks ago, a television producer--he's interested in this issue. Well, this producer said that his brother was out there in the Pacific for six months, was on ships where they had fallout on the ship and they had to hose them down and scrub them and scrub the ship. This guy was involved in several tests.

Hughes: I read accounts of that in Hamilton's papers in the [Ernest] Lawrence collection.

Gofman: Yes, for sure it happened. Well, the reason why this is bad for the government and the defense department is this producer's brother raised a question, wanted to know his dose. He'd been in the Navy and his records show that during that six months he wasn't assigned anywhere. He wasn't assigned anywhere!

So this got this producer fired up. He says, "They can do anything, but when they just make a man nonexistent for six months, that's just too much." So now you couldn't convince this producer that if the United States government said five and five was ten, that he would believe them. He just wouldn't believe anything out of them because it happened in his own family. It wasn't that somebody said, "You can't get these records." If I'd told him the story off the cuff about the records burning in St. Louis, he'd say, well, maybe Gofman's biased or something. But it was his brother. "We don't have any record for him being anywhere in those six months." That's the story.

Hughes: Well, switching again--in 1950 a radioisotope unit was set up at Highland Hospital in Oakland. Did you have any part in that at all?

Gofman: No, I didn't. Was that a collaborative thing between John Lawrence...?

Hughes: Yes, in fact in the beginning at least, maybe longer, it was Donner Lab personnel who staffed the unit.

Gofman: It would have been something I just didn't participate in. I did later do some work with Highland Hospital in connection with getting some of the blood samples. I think I mentioned that early patient that I got a blood sample from in connection with the heart disease work, but that was all. I never participated in the Highland/ Donner Unit. I think I remember something about it....

Hughes: I think it was John Lawrence's idea. Maybe one of the Dobsons was over there. I can't remember now.

Gofman: It could have been Lowry Dobson. Lowry would know about it if you want to ask him. He's at Livermore, as I said.

Draft Deferment, 1955

Hughes: In Hardin Jones's correspondence, which is in the Bancroft Library now, there is quite a bit about your draft deferment in 1955. The reason there are letters is that Jones was in Sweden.

Gofman: He had been there before. I'll tell you what happened; it's very interesting. At that time they were calling up all physicians who had not served yet. The Lab wanted to get me deferred from going. I think they got a couple of deferments. And then there was some change; they weren't going to defer anyone. So I was figuring to go in.

The Air Force was quite anxious to have me come down to Randolph Field to work. They had a program on heart disease and we'd actually taught them all about the ultracentrifuge. So I applied for a commission in the Air Force. I was given a commission of captain in the Air Force and I understood I was going to get called to Randolph Field. So I became a captain in the Air Force in 1954--I think; it could have been early '55. I never got called. And I don't know to this day why.

Hughes: Is that so? There seemed to be a lot of concern.

Gofman: The Lab was trying and I said, "Look, forget it. I don't want to go through this every few months, finding out whether I have a deferment or not. I'm going to apply for this commission and I'll go off and serve for the time and then come back."

I was expecting at any moment to hear that I had to appear. I just have a suspicion that there were some highly placed people in the Air Force who were very interested in the program at Randolph Field on heart disease--one individual in particular, who was highly placed in the Air Force, involved in taking care of the highest Air Force, Army, and Navy brass with respect to heart disease, who thought my work was great. He may have felt it was more important for their program to have me stay at Donner than to go off to Randolph Field. I'm only speculating that possibly that's why I never got called, but I never did.

Hughes: Was John Bugher somehow involved in this?

Gofman: Not in that. Bugher may have been involved during the time when they were trying to get a deferment. They were trying maybe to get Bugher, as head of AEC biology and medicine--

Hughes: His name was mentioned.

Gofman: --to try to recommend to the government that a deferment be granted. But that would be the only way. Once I had told the Lab "I'm going to apply for this commission; just forget it," then all those people were uninvolved, and I don't know to this day why I never got called up.

Hughes: Here is a quote from a letter you wrote to Jones in May 1955. I wonder if it's about the same problem. "With respect to my own problem, may I suggest that you give it no further thought, at least not on paper when you write or anyone else since all will do best that way." You've underlined some of that.

Gofman: I think at that point I already had the commission and was telling the Lab "Hey, don't interfere with my life any more. If they call me, they call me; if they don't, they don't. So just don't interfere." Or at that time I might have had the application in to the Air Force and didn't want them to interfere with that. It would have been one or the other.

Hughes: It was that problem. It seemed to fit.

Gofman: I'm sure it's that problem.

Donner Laboratory Administration

Hughes: All right, something else that I found in the Jones correspondence; again, this is May 1955. It's not the same letter. You're talking about the imminent opening of the new section of the Lab and you went on to say--you're writing to Jones again--"So far no one knows what space will be assigned to anyone, but this is really not unusual for John's type of approach to the problem." What did you mean by that last comment?

Gofman: John had gotten the new part of Donner and at that point we were really a little bit crowded with all the people that were working with me. There was Alex [Nichols] and Frank [Lindgren] and Keith Freeman and Tom Hayes. One thing John absolutely never did, never--and all of us felt that this was just not the right way to run a lab--he never consulted with the faculty members or the leading scientists at Donner on what should be the way we'd allocate new space.

John Lawrence, for example, would decide he'd like to have Alex Nichols on the second floor in the new space. He wouldn't come to me and say, "Hey, I realize you're a little crowded. What would you think about having some of your people occupy a little space?" He didn't do that. He'd pick and choose among the people within a group. He didn't ask me or ask anyone else in some other group. He just decided and he allocated space.

None of us had anything to do with his plans for the new building. I thought in once sense that was unfair. You might say in another sense it was fair. I believe the new building money all came from gifts that John Lawrence had accumulated, but I'm not sure whether that was true for the new building--whether there was any government or university money.

Hughes: I could easily find out.

Gofman: Certainly the early money for the building was the Donners'. John Lawrence got it because he got to know [the Donner family]. He tried to take care of the Donner son. But I think even a big part of the later money for the addition was Donner Foundation money. Robert Donner was the donor.

Hughes: Yes, I think you're right.

Gofman: I think John felt, "Look, this is something I have gotten. It's my laboratory and I'll do it the way I want." But somehow that just didn't seem right to those of us that he left out of the planning. That's what I meant in that letter.

Hughes: In a way it doesn't go along with his mode of operation in general, at least what I understand of it, where he seems to have given a very free rein to everybody. Yet looking at it from the negative standpoint, to disrupt a circle of scientific communication--because it could be disruptive just to have somebody on the floor above or below...

Gofman: He didn't even ask whether it would enhance the work or hurt the work if this piece were physically moved. He just decided, Alex, you'll come here, and Frank, you'll go there. But as I told you in an earlier discussion, with respect to ever interfering in any way with the scientific work, he just didn't. Just absolutely gave you a free hand.

Hughes: Probably the explanation there was that he didn't see it as disruptive to the scientific aspect of the work. He was probably just seeing it from the angle you just mentioned, that it was his building and he was director and that was the way he was going to do it.

Gofman: Yes, this is how he would like it. To be completely truthful about it, I don't think I could ever discern a logic to how he assigned the space. I certainly never had any feeling that he was trying to suppress me by limiting my space. There were some inconveniences and we were crowded, but I couldn't say that John was trying to keep me down. I had virtually the whole first floor of the old Donner and I had some space that some of my people were using in Building 50 up on the Hill.

Hughes: All right, another letter. This is February 1959, to Hardin Jones, who I think was again in Sweden. You say, "Vastly too much purposeless talk goes on in a laboratory like Donner. While ostensibly it is to promote free flow of ideas and a spirit of informality, in truth it represents the easiest way out of a serious work effort."

Gofman: That's a very interesting letter.

Hughes: Well, apparently something had really ticked you off, because this whole letter, as I remember, was on this subject. You said that henceforth you were only going to see people by appointment, that you were not going to tolerate interruptions--

Gofman: In the laboratory.

Hughes: --when you were talking to people. I was wondering if you remembered what prompted that. Was there a lot of gadding about?

Gofman: Well, I sort of thought so. This is now a bit vague, but I thought there were maybe just too many people that really weren't hard at work. I had that impression at Donner. Hardin, of course, was one of the most guilty ones himself, because he did just go talk to people so much. And I had that general feeling. I can't remember that people were oppressing me too much. I did have pretty much an open-door policy. When any of the people in Donner wanted to come and talk, they could just come. I wasn't rigid about appointments.

Hughes: You mentioned that Nichols and Lindgren shared your view, that they were tired of being interrupted.

Gofman: Yes, I remember talking with Alex about it, that he felt that way too. At any rate, there were a number of us who really liked to work in the lab. I had a lot things I wanted to get done in the laboratory, physically in the lab, because by then I had given up my share of the lipoprotein program and was starting to work on the trace elements. I wanted to work; I didn't want to be interrupted. But I can't remember specifically that a given event...

Hughes: Was Hardin just a very sociable person?

Gofman: He was just very sociable, and he never knew how to turn off a conversation. Let me put it this way, and it's not an unfavorable thing to say about Hardin: If I went to see someone, like if I went to see Alex Nichols or Frank Lindgren or anyone else in the Lab, I generally had an idea why I was going. I had some specific business I would like to take up with them, an issue, a thought, or whatever. And when we'd gone over it, we might sometimes speak for five minutes about it or if it was more complicated we might speak for an hour.

When that was over, I just knew how to say "Goodbye, I'll see you."

That's something Hardin never had the ability to do. When he came to see you, you had no idea what he wanted to talk about. And even after you'd try to make some small talk , you still couldn't figure out what he wanted to talk about. He would just stay there and not say anything. With me that's terribly threatening. I don't know why, but I think that I've got to say something. So you make other small talk or try to bring something up. And often, very often...

Hughes: How often?

Gofman: Well, it could happen twice a week, maybe three times a week.

Hughes: When did he do his own research?

Gofman: That's what I wondered about, because everybody said he was in their offices too, doing just... People like Alex said the same thing: How do you talk to Hardin Jones? Now maybe that was Hardin's way of getting a flavor of the Lab or whatever, but there were innumerable times when that happened, when I didn't know why Hardin had come to see me at all, because nothing finally got transacted in the way of a concept or an idea or anything. It was a very grave difficulty.

Melvin Calvin

Hughes: Going on to another individual associated with the Lab, briefly, anyway--Melvin Calvin, who was interviewed by the man who preceded me. I have a quote from the very brief section where Calvin talks about Donner Lab, in a fairly critical way.

He says first of all that basic research never thrived at Donner Lab and then goes on to say, "The understanding of basic science in the leadership just wasn't there." Then I've left out some of the quote; in the next paragraph he says, "They were trying constantly to make clinical applications, going directly from physics to clinical application without much really fundamental biologic understanding in between." I was wondering how you felt about that.

Gofman: I'm not surprised at someone like Calvin saying that. He sort of got himself in the position of saying that most of the radioactive carbon work of the AEC project would be under him and they gave him a section of Donner Lab to do it. There was real unhappiness between him and John Lawrence about that. John thought he was very pushy, which indeed he was.

Hughes: In a Seaborgian sense, would you say?

Gofman: Oh, I'd say much, much more aggressive than Seaborg and much less smooth. Just pushy. Melvin's a very bright chemist. He was also a young instructor when I came to Berkeley, same level as Seaborg.

When I came to Donner, I said to myself, now that I'm back in a fundamental science laboratory, I'm going to approach everything by taking a medical problem and going back to first principles. Even before the biology, go back to be sure I understand everything about the physics, the mathematics. When Rodes Trautman, one of my first graduate students, came to work with me, a very fine mathematician--we were working on electrophoresis--I was sure that everything had to be brought back to absolute first principles of science.

I started that way, thinking that way and planning that way. I made a few of those breakthroughs which led to the heart disease work and we very quickly moved up to the clinical level. I know some of my own students, like Rodes Trautman, said, "Well, why do you want to get into the clinical level? That's not basic enough." I said, "That needs to be done. Some of the others in the group can do the basic things." I changed my opinion. I don't think you can say that you will go out to approach certain problems by first learning the fundamental biology. That's very nice to talk about.

So while I once thought that way myself. I think I was wrong in so thinking and I think those comments of Melvin Calvin are in error. First of all, Melvin Calvin doesn't have the other perspective of medicine. I don't want to sound at all snobbish, but there is something of value in a medical education--it is a perspective on problems that I sometimes think you don't get if you've had just the basic science training, which I had also had.

So I am not critical of John Lawrence, for example, in having gone right to the clinical level. [When] Melvin [says] "They're trying to go right to the clinical level," he's probably thinking largely about John Lawrence; I don't think he was thinking of my program. The fact is that as John surrounded himself with guys like [Donald] Van Dyke and Nat Berlin and a number of others, some of those guys did very fine clinical physiological research and introduced a lot of principles of using tracers to measure key hematologic parameters and physiologic parameters. It was good work, good fundamental scientific work in biology, which became possible because of the ongoing clinical work.

Scientists have a great tendency in retrospect to point out the logical fundamental progression of ideas that led to this or that, but in truth it never happens that way. You really don't know what the best way to start on something is. So I think if I were setting up a medical physics situation *de novo* and somebody said, "Well, what should we do? Should we have any clinical work, or should we start on only the most fundamental biology and see that we work up block by block to apply it clinically?" I would say forget it. Just get some good people around and let them start it the way they want to start it. If they want to start by treating patients, fine; if they want to start by doing nothing but fundamental molecular chemistry, fine--and anywhere in between, because they're finally going to go one way or another. The problem will tell them where they have to go.

I don't think there is any such thing as Melvin--it's a myth, sort of a dream and sort of a self-justification: "If I did it this way and I did great work, they are not as good because they didn't do it my way." I don't think it means anything.

Let me just give you an illustration of where the marriage of those things sometimes comes from and where something truly great emanates. And it's from people who aren't so stuffy. In my opinion, among the truly elegant chemists of history, there are people like Gilbert Newton Lewis, or Wendell Latimer of the department [at Berkeley], or Linus Pauling. These are, as I look at chemistry in my lifetime, the men I saw and have tremendous respect for, with fabulous understanding of the fundamental level and doing good things.

Outstanding of the three of them, I would place Linus Pauling way at the top. I just think he's a chemist of unbelievable proportions. Did the very fundamental work on the quantum mechanical aspects of the chemical bond. In fact, when I took quantum mechanics with Ken Pitzer at Berkeley, we used Pauling and Wilson's book on quantum mechanics as our text. My copy of The Nature of the Chemical Bond, which I've studied very, very carefully and often, I still regard as one of the really great books of all time on fundamental chemistry, more fundamental than the level Melvin Calvin was talking about.

Oh, in 1947, when I started out on this fundamental approach--"I'm going to go back to first principles; none of this approaching at the clinical level"--with Rodes Trautman, we were doing electrophoresis.We were setting up electrophoresis in an elegant, highly quantitative manner. We'd publish some papers on some quantitative minutiae of what's called the electrophoretic system in free-standing liquid, where you use glass tubes and optical systems such as those we had on our centrifuge.

Well, a couple years after that Linus Pauling and Harvey Itano published a paper in Science using [paper electrophoresis]. They said they'd taken the disease sickle cell anemia and they got the hemoglobin isolated and put it on this paper and it migrated at a different rate from the ordinary hemoglobin. Rodes Trautman and I read this paper. We chuckled and said, "For Linus Pauling to get caught on some artifact like that--it's such a sloppy system." We just couldn't believe that he'd even put his name on the paper.

Turned out to be one of the most fundamental discoveries of all time, because everything you've heard of in the field of one gene, one protein, and finding a specifically different protein structure... He approached it in the crudest way: "Look, we'll see if it migrates a little different in an electric field." Now it's known there's one valine amino acid substituted in about 160 in the chain that's the basis of that.

Well, Linus Pauling opened a new world of biology--all the stuff that really finally influenced all kinds of things in genetics, molecular genetics, molecular biology. He opened that world with that one experiment. Now that I think about it, it's probably the greatness of Linus Pauling that he could shift from exquisite quantum mechanics and measuring things down to one angstrom or one volt to doing this crude experiment, but seeing what it meant. It just opened a world.

I suppose Melvin Calvin would say, "Well, you see, these guys are doing things like that experiment with paper electrophoresis, crude things instead of fundamental science." But it opened a world.

Hughes: Well, that answers that question pretty well, I think. An incident told to me by Ron Kathren--does that name mean anything to you?

Gofman: Yes, I know Ron Kathren.

Hughes: He called me out of the blue, sort of a la Hardin Jones. Somehow he'd heard that a project in medical physics was going on. He said, "Have you heard about this incident where two miners inhaled radioactive iodine [near] the Nevada test site?" He didn't remember the date. They were apparently rushed first to Lawrence Livermore and then were subsequently examined on the whole-body counter at Donner. And you were involved, he said.

Gofman: Yes, I was involved in that episode and, you know, Sally, I don't remember any of the details. I remember these guys that got exposed.... I think it was radioiodine that was at issue.

Hughes: Yes, that's what he said. He said radioiodine gas near the Nevada test site.

Gofman: Yes. I just draw a blank.

The Division of Medical Physics

Hughes: You were from the very start a member of the Division of Medical Physics, which I believe to this day has had space problems, severe ones.

Gofman: Yes. See, the Division of Medical Physics has at no time ever had any authority over Donner Lab. It is just sort of there. We really have to think of why the Division was there. Medical physics has grown to be respectable subject. It has a valid place, namely the application of physics, chemistry, and mathematics to problems of medicine, where biophysics is the counterpart, the application to problems of biology.

Let's look at why [the Division] was created. It was created in the University of California because they wanted to have an academic position for John Lawrence. So they created the position of medical physics. Well, that gave them university legality.

Then they wanted to attract some additional people to the staff of the Donner Laboratory who might be a credit to the Laboratory, but who said, "Why should I come to work at the Donner Laboratory on an AEC-funded enterprise with no academic standing whatever? I can go elsewhere and maybe have a professorship to work up to." So having the Division, they had a vehicle for bringing people to the Laboratory and giving a certain number of them, that they really wanted to have, an academic position. So Jones and Tobias got academic positions and I got the next one. For quite a while that was all of them, I think. Hamilton had a position.

I remember one thing, which by the way has cost me a great deal of money. I'll mention it to you now and I'm not bitter about it, but I know Helen Jones was quite bitter about it when she found out after Hardin's death. When I came, I was offered an assistant professorship in the University of California, a certain number of dollars. You worked in the summertime, too, because you were working in the Lab.

I noticed that the appointment said half-time university payment and half-time Donner Laboratory budget, and the summer was paid out of the Donner Laboratory budget. I said, "What's the meaning of this, John? Is this a half professorship?" He said, "Oh, no, it's a full position as an assistant professor in the Division of Medical Physics, but for the convenience of the Laboratory, since we have the outside funds, we're paying half of it from those funds. And if at any time the AEC should stop providing those funds, we have assurance that the university will pick up the other half."

But there was one little gimmick that I didn't think too much about: They didn't take retirement out of the half that was paid out of the AEC in the summer. I raised a question a couple times in subsequent years and talked with Hardin about it. We never could get that resolved, and they didn't take retirement out. So finally I ended up with about forty percent of the retirement that a professor would otherwise end up with, because it just had never been taken out and there was no mechanism for it. So it really wasn't a regular position.

But that's a secondary part. Helen Jones ended up getting very much less than she thought out of Hardin's retirement. She was just shocked. I met her about two months after his death and she was just furious and shocked and I said, "Well, Helen, I knew about it. I'm getting a very much lower retirement than I thought I would." They even tried to gloss over that retirement, that it wouldn't matter, but it did matter.

Hughes: Do you think it was deliberate?

Gofman: I think it was one of Bob Underhill's manipulations mainly: "Sure these guys want to have these funds, but I, Bob Underhill, the protector of University of California funds, am not going to give away anything that I don't have to." So by handling the retirement issue this way, he probably felt, fiscally, that he didn't have an obligation. So I don't think it was underhanded in that sense.

Hughes: What about this situation of having a Division of Medical Physics, an academic unit, and a government-supported lab headed by the same individual and without, as far as I can tell anyway, much discernment between the two institutions?

Gofman: I think it was unfavorable if you look at it from the point of view of the academic side, unfavorable from the point of view of Medical Physics, because it really never had a home. It was at John Lawrence's whim as to what it would have and how much it would develop. I don't think anyone in the Medical Physics Division could have really thought to undertake anything that would be antithetical to the Donner Laboratory government-supported program. That just wouldn't have been tolerated. And that, academically, is very bad.

In other words, I think, within Donner Laboratory, if some member of the academic faculty had decided he had some research that was just going to reflect horribly on the Donner Lab government program, it just wouldn't have been tolerated.

Hughes: In that light, do you remember any static--I'd say it was probably the early 50s--when Hardin Jones came out with his statements about the uselessness of radiation treatment, of breast cancer particularly?

Gofman: There was some static, but I don't remember too many of the details about that. But, yes, I think John was very worried. John worried. John was a worrier. He worried about the possible impact on the Lab and the funds from anything that anyone would say. So I'm sure he was worried about Hardin's thing.

Hughes: There was a letter. I don't have the exact quote now. It was John Lawrence writing to you. You were going to give a talk at Davis. I'd say it was probably in the 50s. He was saying, "Well, of course, John, you're going to take a moderate line on this issue" or "you always do" or something to that effect.

That letter could have been read two ways: either he was saying yes, I know I trust you, you always have a reasoned approach to things; or it ws a semi-threateneing sort of thing, where he was saying, well, now, this is what I want, this is the tactic that I want to take. You were talking about fallout.

Gofman: Wasn't this thing at Davis?

Hughes: Yes. It was something connected with radioisotopes getting into the milk supply.

Gofman: Let me think. There were two things. What years was the first Governor Brown? During part of his tenure, Governor Brown appointed a committee in the State of California to concern itself with all uses of radioactivity. I think it was all for the university, not for outside the university. John Lawrence and I were both appointed to that committee.

It was of course a dud. We went up one day and Governor Brown just chewed the fat with the committee and talked about anything but the work of the committee. They said, "Oh, the photographers are here," and so then we went into a serious session with him explaining why the committee was needed. The photographers left and the conversation went back to the other things and absolutely nothing every happened with that committee, nothing. It was just purely for show. No, that wouldn't have been how I would have been involved, and I just don't remember the talk in Davis.

Hughes: Can you interpret the tone of the letter? Would Lawrence have ever issued you orders, so to speak?

Gofman: Not orders. I'll tell you what I do remember. There was one period when both Lawrences were concerned about something I said. Adlai Stevenson ran for president in '52 and '56. In '56 he was making a big issue of the fallout question and weapons testing and the harm it might do. I went to Adlai Stevenson's house with Mrs. Mary Lasker, a very wealthy New York woman who's been on the heart council and cancer council, who is a big fan of my work.

I was in New York, talking at the New York Academy of Medicine and participating in a symposium. Mary called me up and said, "What are you doing this weekend?" This was a Friday. I said, "I'm going to the meetings." I'd already seen her. She said, "How would you like to go to Adlai Stevenson's house with me? He's going to run for the presidency again." This was about April or something like that. She says, "He doesn't know a damn thing about heart disease and you could educate him." So I said, "Mary, if that's what you think would be advisable, I'd be delighted to go."

So within about two hours we were on an airplane from La Guardia to Illinois, where Adlai Stevenson picked us up in his car, and went out to his ranch. I think it's a place called Libertyville, Illinois. It was a lovely farm he had. She and I were his houseguests. He had already separated from his wife.

He was a charming host and we spent the weekend talking with Adlai Stevenson over all kinds of things. He mentioned his concerns about the fallout thing and talked about heart disease. Then at some meeting in Marin County when the campaign was on I did speak for Adlai Stevenson, and I backed some of the things he said about fallout hazard. And that came back to the Lab.

My recollection is that Wally Reynolds, the business manager of the Laboratory at that time, made some comment to me about did I think that that was the right thing for someone in an AEC lab to say. And I said I thought it was the right thing for anyone to say scientifically. But the fact that Wally Reynolds said it to me--Wally was Ernest Lawrence's right hand--might have meant that Ernest wsa unhappy about that too. But I do not know how to tie that to this letter from John Larence about my comments at Davis.

Hughes: Well, it could have been a perfectly innocent letter.

Gofman: There was one later thing. These dates sort of slip around in my head. On some of these things, when I say something happened in '55, I could be wrong by a year or two. For example, I'm not even sure now of the exact year when I got that Air Force commission. I just could be wrong, that's all.

But somewhere along the line, I think '56 or '57, William Libby was an AEC commissioner. Libby was a young instructor when I was a graduate student of Glenn Seaborg's. I had looked at Libby's work. He became an AEC commissioner. He was a gung-ho commissioner for testing--still is, by the way.

Alice Stewart's work on the occurrence of leukemia and cancer in infants in utero had come out and they were all very worried. I can't remember the connection, whether John or Ernest had asked me, but in some way I went back to see Libby about it. I looked at it and I told them one of the interpretations was that this work indicated that those low doses of radiation were productive of leukemia and cancer.

One of the ways to look at it was that there was this possibility that the women who were destined to have a child with cancer, leukemia, had something else constitutionally wrong with them that led to their getting the x-rays. I didn't know the reason but I said therefore it isn't proof. That is the sort of thing I said about Pauling's work in '57, that he has the data for high dose radiation but doesn't have the proof for the low dose.

I think I was wrong in both cases, when I was talking with Libby, and saying Alice Stewart doesn't have the proof. I think what we should have said, in view of the fact that there's a one out of two chance it's that way, is that all action should be based on the unfavorable thing; otherwise we're experimenting on humans.

I'm just having very, very great difficulty fitting together that criticism from Wally Reynolds about my speaking in behalf of Adlai Stevenson, then my saying there were other explanations with Libby. I just don't remember.

Hughes: Would you have any correspondence dating back to that period?

Gofman: I probably did, but in 1973, when Margaret and I came back from Livermore, we just tossed out boxes and boxes. If it exists, the only correspondence from that period would be, as I said, in that record-keeping division of AEC or now Department of Energy. They commented that they had some boxes of my records and I never went to check them out. They may still be there and there might be correspondence from that period there. But I'm just confused about those.

There was criticism from Wally Reynolds about that Adlai Stevenson talk, but I don't tie that to John Lawrence's letter. I can't even remember the Davis talk, except I remember I went to that Ploughshare symposium at Davis and gave this talk that happens to be sitting in this notebook.

Hughes: All right, another letter. Are you getting tired?

Gofman: No, no, that's fine.

Hughes: Again, it's May 1955 and you are still writing to Hardin Jones--this is the same letter. You referred to the "trivial, ridiculous crises which have characterized the place"--Donner Lab--"from the first day." Do you know what you were talking about? Because of some crisis, which you didn't name, which had come to his attention, you said [to Hardin Jones], "Oh, don't contemplate coming home." He apparently was just about to get on a plane and come and solve this problem.

Gofman: Hardin?

Hughes: Yes, and you were writing back to say don't worry....

Gofman: Was he in Scandinavia at that point?

Hughes: Yes. Part of this letter was this statement that I just quoted, which may have been in connection with this space allocation business.

Gofman: There may have been things like space allocation, or fund allocation out of the AEC budget; but what might have seemed like a great crisis to me at that time... There were always concerns about whether next year's budget was going to come through.

Hughes: Well, you say "trivial, ridiculous crises."

Gofman: Well, that's what I really think they were, because my recollection is, as I say, some question about space allocation or whatever. I don't remember a single thing from '47 to '55 that this would cover where John Lawrence and I had any problem of any kind, or any crisis he might have created was anything I would be concerned about.

Talk about crisis: Bob San Souci was business manager, and I know the kinds of things that come up. He was the business manager at Donner before

Jim Born sort of came in and took over being the official administrative head. Let me give you an illustration. He'd come down to me with a long face and say, "We're in big trouble." I'd say, "What's the problem, Bob?" He'd say, "John Lawrence has brought this guy from Europe, given him this space to do this work in his clinic, and he's come and told me to figure out a salary for him." And he says, "I can't do that. We got a rule that the AEC funds can't be used and John Lawrence has come and said to do this. How am I going to do it?"

That would be a crisis. John had just created a crisis. Or San Souci would come and say, "We don't have a dollar over what we need and John has just told this guy he can go buy $20,000 worth of so-and-so, and then says, 'Go do it, Mr. San Souci.'" Those were the crises.

Hughes: Why would he come to you?

Gofman: Just somebody to lend an ear. Not that I had any official position. He just felt that he could never get a rational answer from John. He just didn't feel he had the power to ask John or to say to John, no, you can't do this.

The Institute of Medical Physics

Hughes: [I saw a reference in a letter to a company with which you were associated. Please tell me about the company.]

Gofman: We were approached because a lot of doctors were asking about getting bloods done on the ultracentrifuge in about 1951 or 1952.

Hughes: The letter is dated March 2, 1955.

Gofman: Right, yes. The men who owned the company, which was called Spinco, were Edward Pickels--Pickels was the scientist who built the ultracentrifuge--and a man by the name of Maurice Hanafin, who formed the company. The third man in the company was Ralph Sherman. They got interested and thought maybe they ought to set up a company to offer this blood analysis, and would we help? They set up a company called Belmont Medical Lab. All kinds of doctors were asking us to do bloods at Donner and we were doing a lot of them. In fact, John Lawrence was often the one who told the doctors, "Yes, you can get the bloods analyzed by John Gofman." That would be a minor crisis.

So they set up that lab and pretty shortly it became operative. But it was not a particularly attractive enterprise financially. They didn't really particularly care about it. So they said, well, why don't we set it up as a nonprofit thing and offer it as a service, because then you can get some funding from foundations and so forth. So they switched it over--actually their company provided a lot of the money and provided the equipment--to a nonprofit corporation.

They selected the name Institute for Medical Physics. The thing it was doing was providing the ultracentrifuge analysis for lipoproteins, and they were thinking of providing some other tests. One of my former students, who had not continued to work for his degree but had been a technician, went down there to work for them. From '53 or '52 maybe, [the films were] already in the Institute for Medical Physics. The analyses I checked over to be sure they were doing them right, and I wrote some of the scientific literature for them. I received pay per hour when I did the work on checking the films, so I did get paid for my efforts.

They had a man by the name of David Spector, whom Ralph Sherman knew and thought was a great organizer. I was able to work with him effectively, but finally there were a number of things we didn't like about Spector and we disassociated ourselves, Hardin Jones and I, from helping the Institute for Medical Physics. Spector continued operating it, with a different board of directors, for at least several years thereafter, and then I lost track of it.

John was unhappy about the choice of the name Institute for Medical Physics, and kept making comments and suggesting that the name be changed and so forth.

Hughes: Because he didn't want the [Division of Medical Physics] to be associated with it?

Gofman: Yes, right, and thought there might be some competitive aspect. But it never got to be anything very large. I think they got to run as many as a couple hundred or so tests per month. There were managing to stay afloat, is what it amounted to. Maybe they got up to 300 or 400 blood tests some months. A lot of physicians were using it.

I had a lot of correspondence with physicians who wanted advice on how to use the results. I think it was a good service that should have been provided. I'm glad it was provided. If I had been so inclined, I think I'd have favored setting up laboratories of that capability much more broadly throughout the country. Having to ship from one end of the country to the other was an inhibition, though a lot of doctors did it. I think more should have been done with that. It would have moved the field along faster, I think.

Academic Positions at Donner Laboratory

Hughes: Another question: It seems to me that there was a problem providing academic positions for physicians. I'm thinking now particularly of your student Nat Berlin, who from every account I've heard was extremely able--

Gofman: Very able.

Hughes: --and was relied on very heavily by John Lawrence--

Gofman: Extremely so.

Hughes: --in all senses, I mean in the scientific sense and the administrative sense, and then eventually left.

Gofman: He was John's right hand, and he left because they just didn't, couldn't--getting an academic addition was very hard, very hard. The reasons for that were that in the academic community in Berkeley, medical physics was not highly regarded. They sort of thought it was not really a science. I think if it were a department, if the circumstances had been such that it had grown up in a medical school, additional appointments for someone like Nat Berlin would have been a breeze.

But somehow they were very difficult in the Berkeley atmosphere. John apparently didn't have enough clout--well, he'd have to go through Raymond Birge and through the [Academic] Senate and all that. But in any event I know it was nearly impossible to get additional academic appointments. And Nat Berlin did go off to the National Cancer Institute.

Hughes: What effect did this adverse climate have, do you think, on the whole development of medical physics in Berkeley?

Gofman: Well, I think that the adversity is a phenomenon that's a thing of great unhappiness in many, many places around this country, where you have this dichotomy between an academic arm and a large laboratory or department. But that's true today in many places, Sally; it's true in medical departments, in pediatric departments, where you have a large research enterprise going on funded by outside funds and a small academic enterprise funded by institutional funds, either private, such as Stanford, or state, such as the University of California.

Oftentimes the professor in charge wants to have a big enterprise going on and he can get the federal funds and he does it. Often, unstated, there is sort of the lure or the expectation that the scientists who work in the larger enterprise will in time get one of those academic posts. Well, we're working on getting more academic positions, which somehow never materialize in numbers comparable to the number of scientists, which I think leaves at all times a very unhappy situation.

It was true at Donner. We had to push to get Alex Nichols to have an academic appointment. Somehow that worked out. With Frank Lindgren it was never a problem because he was terrified by the idea of an academic appointment.

Hughes: Oh, is that so? Teaching?

Gofman: He just couldn't face teaching. Yet it turns out that in lectures at a distance, in scientific societies and symposia, he's a very effective educator--likes to go do it. I practically had to sit and hold Frank's hand for a month before he went off to his first scientific meeting to present a paper. So he was very happy to be in Donner, do his research and not have an academic appointment.

But I think there were many who weren't, and there was a very serious problem of morale. Nat Berlin was an illustration. I think it had a big effect on his morale, not to be able to... He was relied on heavily; he thought he was doing good work; he was handling a lot of things; and yet there was no way of his having a position that recognized his permanence.

From the point of view of the individual who doesn't have the academic position, [he] looks upon it as a sense of impermanence; that is, [he] looks upon the academic appointees as the permanent people. And, to some extent, that works. I don't know, maybe it would be a good idea to abolish all tenure and have everybody at all times subject to being fired. I don't know whether tenure is a good or a bad thing. I'm sure when I was active in the university I always thought it was a good thing, but I don't know.

But certainly the non-tenure positions, in the face of a joint academic division, were an unfavorable thing and tended to create an attitude of two classes of people. I'd rather never see that. There's something wrong with it. If you're going to have an academic department I think maybe you ought to have people come for a post-doctoral year or two, three, a fellowship or a graduate student working for a degree, but I think it's sort of a mistake to have scientists stay on in a post where they are not on the same level, and can't hope to be, as those in the department. There's something wrong about it. I think it's detrimental to their career.

Efforts to Establish a Medical Complex at Berkeley

Hughes: Yes, which more or less leads into the whole issue of a medical school and hospital at Berkeley, which seems to have been a theme throughout the years--the various attempts made over the years to have one or the other or preferably both. Were you involved in any of that?

Gofman: Yes. I didn't have any real clout of any kind. In the years before that final decision as to whether the medical school in San Francisco would move to Berkeley or the first year would move to San Francisco, I was very hopeful that it would go the direction of moving to Berkeley. Perhaps partially selfish, but I thought it was reasonable, especially since I joined a Medical Physics Division and the whole idea for my background was that basic sciences ought to be brought into medicine more.

I thought there was a big serious defect that Berkeley didn't have a medical center right there. It's just not the same. The atmosphere and the flavor, and the interactions that I might have gotten engaged in in work on lipoproteins had there been endocrinologists walking around in a department of medicine right there... It just would have been totally different. Of all the things I might say I missed in my Berkeley experience would be the fact that it wasn't part of a medical center. You can say, well, is it all that hard to be 30 miles apart? Well, it is, in a real sense. So I was certainly on the side that favored it. I don't remember if I specifically had any opportunity to try to influence this decision.

Then that idea lost and the first year [of medical school] moved out [of Berkeley]. Then they for a number of years talked about establishing another medical school in Berkeley. And they did establish the first year of some program [at Berkeley] for a while. Is that still going?

Hughes: It's still going. To tell you the truth, I don't know exactly how it works, but there is some program that is heavily weighted toward the basic sciences and I think it's under the aegis of Public Health or something.

Gofman: Oh, is it? I didn't know who was running it.

Job Offers in the 1950s

Hughes: Did it ever cross your mind because of these handicaps, particularly in the lipoprotein work, when you were definitely getting closer and closer to the medicine aspect of it, that perhaps the proper place for you was at the medical school rather than in Berkeley?

Gofman: Well, it crossed my mind quite a bit that the proper place for me was in a medical school. But it didn't cross my mind that that would be at U.C. Medical School, because I had a very low regard for U.C. Medical School's department of medicine, though I was a member of it. As I said, I never was very active.

The University of Miami offered me a position in 1953 as a professor of medicine. They were essentially trying to buy the lipoprotein work. And it had some fairly attractive features; the one thing I did consider attractive was that it was at a medical center. I finally elected to turn it down. It would have involved a big salary increase compared to my position at Donner, but I guess when faced with it, Miami didn't look terribly attractive to me.

In about 1954, maybe, the position at the University of Washington at Seattle in biochemistry opened up. They came down and said that I was being considered for that. And I discouraged them. I said I didn't think I would come, and I didn't. They were fairly insistent at first, told me what opportunities there would be. And I said no, I thought I was quite well settled at Donner and happy there. I said I do miss a medical school. At any rate I discouraged them, told them not to consider me further, and Hans Neurath from North Carolina [Duke University] took that job.

Not too long thereafter, I wondered about the wisdom of that decision. That, in retrospect, maybe would have been a sensible move to make, because it was the chairmanship of the biochemistry department in an elegant medical school in a lovely setting, Seattle. So I could have gotten the clinical thing and still been in fundamental sciences, chairman of biochemistry. So I think that was probably an error, not to consider that one seriously. But it crossed my mind many times from 1950, 1951 on that this sort of work would be better done in a medical center.

The Heart Monitoring Device

Hughes: The heart monitoring device--what is the story behind that and when did that all occur?

Gofman: That has an interesting story that's a sad story for medicine, in my view. I still have those little monitors, a couple of them. I was working at Donner and two friends of mine, Arthur Biehl and Robert Mainhardt, who later organized companies very successfully, had a small company, and they asked me to come and give a talk on what was important to do in medicine that engineers might help with. I said okay. So I came and talked to them.

From my work on heart disease I was convinced that the most important thing you had to do until we fundamentally solve atherosclerosis was to work on some way of preventing sudden death. A fair number of the people who die suddenly of atherosclerotic coronary disease, if you got them over that one little period of electrical instability, they might have 10, 20, 30 good years. It wasn't that they were worn out, that the atherosclerosis was so severe that they were ready to die; they just weren't.

So I said, if we had a device that would let you know if there were any irregularities building up toward ventricular fibrillation--there was then a crude literature, not much, that suggested that multiple ectopic beats, that's extra beats, would lead to the fibrillation--that it would be a very useful device. I'm not talking about hospital use; I was thinking of ambulatory use. I said maybe you could do it by the fact that the deflections are not the same in the cardiogram for the ectopics as they are for the normal beats.

Well, there was one guy in that audience, Robert Chapman, who was an engineer working with them. He apparently heard the talk and thought about it. He called me up about three months after it and said he'd thought of a way you could build a monitor that would do this and that Mainhardt and Biehl weren't interested in doing anything with it, so he wanted to see me. So he saw me, maybe '62 or '63 by the time he'd fooled around with it, drawn up some diagrams and so forth. He tried to sell the idea to some companies. Nobody was interested.

Then a fellow by the name of Robert Roy, an entrepreneur, a businessman--Chapman got hold of him, showed him the idea. Roy said he thought he could raise some capital for it. In exchange for that, he wanted 50 percent of all the rights. So we set up an agreement. I said, "Look, I only provided the idea." So the agreement was Roy would get 50 percent of anything, Chapman would get 37 percent, and I'd get 12.5 percent for my ideas.

Well, nothing really happened until 1969 or 1970. Oh, we saw people and talked to people, then two years later he'd have another person, but nothing happened on it. By 1970 Roy saw a way with some group that was a defunct mining company in Nevada, called North American something-or-other, that you could put this together to advantage financially and maybe raise some capital for the instrument. And so they did, and put it together as a new corporation called Cardiodynamics. Its function was to develop this monitor. It had this existing mining company, which was merged into it. So the people at North American got half the shares of the new company and the other half was divided three ways. By that, I got one-eighth of the other half.

They got hold of a guy by the name of Richard Lawhorn, a superb electronic engineer, and he started to make breadboard models of a monitor along the lines of the patent that Robert Chapman had developed. I was busy at Livermore, not interested in getting involved in any of the details. I said, "If you have a question now and then, okay." But I was just up to my ears with the whole fight over the radiation standards thing. I said, "Besides which, you've got to have some clinical setup anyway." So they made contact with Don Harrison--he's the head of cardiology at Stanford. So they got it set up and they were going to start testing it on patients. Chapman kept telling me that everything was going fine with Harrison. I said, "Good, then you don't need me."

Well, near the the end of 1971, beginning of 1972, they were able to sell some additional shares and got some money into the company so that they had operating money. In fact, I thought they had too many people. I was on the board of directors. That was peripheral; we didn't look at it closely. Lawhorn had worked through to about the fifth iteration of the model and they were telling me the work at Stanford was going very well. They were planning to actually have the thing marketed within a year.

Chapman said, "Harrison's having some trouble with false alarms and we can't figure out what it is, so would you look into it?" I said okay. So I went down to see Don Harrison. We talked over what the problem was. I'd had this idea of what ought to be done, but hell, I hadn't played with any of the hardware of CCUs or cardiograms or electrodes, so it was all Greek to me. But I saw the problem: They were getting awful false alarms. I started to look into it and I spent about a month and a half and I realized this goddamn thing is so far from the marketplace it'll take a miracle to get the problems resolved in a year.

But I started to work on them. The first problem was the electrode. Remarkably and accidentally, I really think, in about four months' work I figured out why the electrodes weren't working right and designed an electrode, which we patented, by the way, and which is being widely used clinically now. It solves most of the major problems of false alarms in CCUs if you use the electrodes the way I worked out. But I had to work it out. I had an abraded chest, as I had abrasion to prepare the skin; I was doing all the experimentation on myself.

That was one problem; I got that worked out. Then there was a problem that the monitor could only work if your heart had a certain position in your chest, if you used just standard positions for the electrodes. So I had to work out a whole system of mapping the chest cardiographically to pick out two positions that would fit the model.

Well, by some miraculous set of things, we got those two things solved by the time they put the monitor out on the market. Chapman had had some people doing market surveys and they'd interviewed doctors and gone through all the things that companies go through in market surveys, and he was just sure that sales were going to take off like crazy.

I wasn't, especially when I visualized Dr. Joe Doakes having to map that chest: he isn't going to want to spend that time mapping that chest. I said, "There's going to be a hell of a problem getting medical acceptance. Maybe if you had it such that you could stick it on the chest anywhere, without caring where you put it, maybe then you'd get acceptance." Lawhorn said, "I'll get to that stage, but it'll take a lot more electronic work." It wasn't at that stage.

It was a nice instrument. By that time I had worn that instrument continuously for about 10 months and I knew it functioned beautifully if you knew how to use it. It was a beautiful job Lawhorn had done.

Well, the acceptance by the medical community was just about zero. I remember there was this group--they're up on the hill here in the medical center--called Electrocardiographic Associates. They read electrocardiograms that are put in over the telephone. Our device you could hold up to a telephone and transmit the cardiogram, in addition to warning the individual. So he could not only be warned audibly, but he could transmit his cardiogram to a center.

This group got interested in the monitor. So I went over to see them and I showed one of the doctors there how to map the chest. I saw he wasn't listening to me very carefully; he'd just say "Yes, yes." And so I left him some monitors. He was going to put them on some people to try it out. He called me back in about ten days and said, "That fitting out a patient, that's a bear." I said, "What do you mean?" He said, "Well, I tried to put it on this patient and the damn thing kept going off and sounding alarms all the time. I finally gave up and told him I couldn't use the monitor." Well, the fact was, he hadn't listened to what I said and was so sure he knew. And that was a problem.

So they were getting nowhere. Then their capital was running downhill because they had many people working, assuming it was going to sell and start to generate a cash flow, which it didn't. There were a couple of looks at the company by major corporations to acquire this whole thing. One of the lessons I learned is undercapitalization and underrealization of the time it takes to do something are the most serious reasons for business failures.

Finally a group in Oregon, a couple of guys who were entrepreneurs, decided they'd take it on, with the idea of setting up a clinic and offering it as a service. We made an agreement with them and they set up a clinic in Eugene, Oregon. I went up for six weeks, for about three days a week. A CCU nurse was in charge, Betty Hollinger, a very capable woman. She learned exactly; I gave training sessions to her and other technicians.

Some of the physicians were sending patients in. Betty Hollinger would fit out the patients. They'd come into the clinic and be fitted out. Or for a city 20, 30, 40 miles away from Eugene, she'd drive out to fit the patient out. Within the first month there were two cases where I'm positive we saved their lives with the monitor and the physicians were very delighted. But the rate of putting patients on was just too slow for their financial things, and finally they had to fold up--not because the clinic wasn't doing well; it was doing beautifully.

[The heart monitor] just died there and it's never been resumed. So what was needed was that next couple of iterations and development by

Lawhorn that would have made it possible for any physician to just put it on and forget about it. You had to go through the mapping procedure or the thing would continue to false alarm. It never false alarmed on me. It didn't false alarm on the people I fitted it on. So that's where the monitor went. Sad.

Hughes: It is a sad story.

Gofman: I think, widely used, it would have been cheap by now; maybe could be saving ten, twenty thousand lives a year. Someone else will reinvent it and it will become a part of medical use. I have no doubt about it. But I lost interest and haven't done anything with it at all. In the last stages of trying to keep it afloat, I tried to help on business contacts. That's not my arena.

Scientific Method

Hughes: Well, on to a very large question. We skirted around the edges of this and we've even talked about it a little bit.

How do you do science? I remember a remark that you made, I believe in the first interview, when you were describing just sitting down and reading the literature when you first came to Donner Lab and thinking that what you wanted to do was work on the cancer problem. But the ideas didn't come. So then you happened to read the Pederson paper and thus the whole lipoprotein business followed.

Is that common? Is it that cut and dried, that you say, well, "Eenie, meenie, this is the problem I want to address," start reading, and the ideas come?

Gofman: Well, once you're in a lab working--maybe you start working in someone else's lab as an associate--then I think ideas occur partly as a result of reading, exposure to the subject, and partly as a result of the accidents of what you're doing. That's where your ideas come from: the things that go wrong, the things you see in the periphery of the field you're working on and you say, hey, that's not explained. You might defer it and not do anything with it, but then you later go back to it. That's not hard. I think once you're in the laboratory having gotten started, just one thing always leads to other ideas if you keep you eyes and ears open for the accidents in the lab and so forth.

Hughes: That happens if you're good. Thinking back to our conversation at lunch, there are cases of people who don't seem to have that ability, whatever it is.

Gofman: Yes. I think that's a matter of the individual. If the individual is not attuned to seeing the prospects of a certain thing that looks wrong, they say, well, that's wrong, and just set it aside. But I think the better scientific mind is the one who says, now this has an explanation. Why did that thing occur, this apparent oddity?

See, showing you the quality of minds: I looked at Pauling's thing [and decided] that's probably just a sloppiness of technique. He looked at it and said, "Hey, I might have a different molecule here." I think that same discovery in my hands, I'm sure I would have lost that. He didn't.

And I think it would have been in better hands [with me] than a lot I know. Yet I think I would have lost that and he didn't, which shows you one of the ideas of how you do science. Many things are laboratory errors and don't mean anything more than just a laboratory error. Somehow you've got to get a sort of feel for which ones ought to be followed up.

I think there are some things on how to do science and how not to do it that I do have a suggestion on. Besides the fact that, as I say, getting started is hard. It was hard for me. I wasn't going to go back and work on nuclear chemistry as I had in the project during the war, on the U-233 or the plutonium work. I was committed to work in medical things. I just honestly didn't have an idea as I thought about cancer and read like hell, just didn't have anything that looked to me like an idea.

But the ideas I do have are these: It may be arrogant, but it never occurred to me to work on a small problem, as I saw a lot of people around Donner [doing]. How do you measure blood volume? How do you measure liver blood flow? [That] was Hardin Jones's thing. I helped a little on Hardin's projects during that summer period I told you about. Well, it never occurred to me to say how do you do this?

But to me, when I decided to walk in as a career and try to do something in health, I said, well, what are the big medical problems? The big medical problems are cancer and heart disease. Most people would say that's just the wrong way to go at things. You don't take a big field and say these are the problems, let's work on them, because no one has a way to tackle those problems. Perhaps out of my ignorance, I decided that was the way one looks at it. I don't want to measure blood flow or liver flow. I haven't any idea why I'd want to know liver flow. But I do want to know why cancer occurs or why heart disease occurs. So that seemed to me a rational thing.

It's true with the liver flow you have a way of looking at the problem directly. With heart disease and cancer, you had to think up how in the hell to even get into the field. With cancer, I couldn't think of a way. So I said, what about heart disease? Then I decided to read about how the blood lipids are transported and Pederson's trouble with the study of blood. It was just one entree. It might have been a total flub and might have led nowhere in the heart disease problem. I might have then looked for another approach in heart disease or said maybe I ought to try another look at the cancer thing. Or what I might have done after a couple of years is said, "Hey, I don't think I have any ideas how to be effective in research at all. Maybe I ought to go into clinical medicine and practice." That could have happened too.

But as I look back on it I would say again: Pick a big problem, an important problem, and see if you can make an inroad somewhere. I don't think it matters as much how you start on the big problem as it matters to start. Because once you start approaching it, if you do keep the big picture in mind, you'll start either from your own observations or what's not working out in your work and what you read about others'. I think that will get you into the field in an effective manner. That's one thing.

It may have sounded arrogant to say I'll pick out the biggest problems in medicine and start working on them. Most people would say that's arrogant and foolish. We made a dent in the heart disease problem. We didn't solve it; I was more sure we'd solve everything in a shorter time than has turned out to be the case, but we made a dent. So it wasn't a totally foolish effort.

I remember Professor I. Leon Chaikoff, a pioneer in the application of radioisotopes in physiology, who did a lot of work on atherosclerosis, by the way. Chaikoff taught me physiology when I was in medical school. He came by Donner one day to see Hardin Jones and I met him. I guess I was there about six months. I had begun to work on atherosclerosis, since I'd passed my three months of doing nothing on the cancer problem. He said, "Well, how do you like it back here at Donner?" I said, oh, fine. So he said, "Well, what are you doing in the lab?" I said, "I decided to start work on atherosclerosis."

If there was one thing he was, Chaikoff was a positive individual. He says, "A terrible idea." I said, "Why do you say that?" He said, "There's just nothing, no way to approach that problem. I think it's the worst kind of thing a young man can get into. Forget it." I said, "But you're working on atherosclerosis." He said, "Yes, but I'd advise you to stay out of it." I don't think he was trying to deflect me from the field, but he was very positive. Maybe for about half an hour I was discouraged: "It's a terrible problem. Stay out of it." But that was just Chaikoff.

Hughes: Well, he wasn't approaching from the lipoprotein standpoint anyway.

Gofman: No. We later did some lipoproteins for him on some of his animals. So that's the first thing: I think you should pick a big area and try to get some way of getting in on it. The hard part is getting a feeling that even dirtying your hands is worthwhile.

The second thing, which again many people would disagree with, is that I think it's a good idea to change directions totally at least once and probably several times in your career, if you want to do good science. Pauling's a remarkable example of a man who just went from one end of things to the other, several times and effectively.

Those who stay in one thing too long, I think they lose their ability to see broadly. They contribute and they add to that body of knowledge that's awfully convenient to have when you're in the library wanting some information for yourself. The fact that they stayed with it and did the dirty work of just following that one thing through makes that paper available to you as a user. But I wouldn't want to spend my life doing that.

Maybe I dropped out of the lipoprotein work a little too soon, because I did come back a couple times to do things in it. When I came back to do some more things, I realized there were still some problems I could have maybe made some further progress with. And there are a couple that I still want to do right now, that I still think I'd like to do.

Hughes: What about the multidisciplinary aspect? I mean, both you as an individual, a chemist and a physician, and working for a good segment of your scientific career in a laboratory that was set up as a multidisciplinary institution. What value, if any, have those two perspectives had for your science?

Gofman: Well, I think they're both important. I like the idea that the Laboratory was multidisciplinary and that there were people approaching things from very different backgrounds and viewpoints. I felt that was favorable and a good atmosphere. For myself, I value very highly the opportunity I had to study chemistry for several years and then to work in the war years on that chemical project. I just think it made all the difference in the world to the way I looked at problems when we were working on the lipoproteins.

Hughes: You mean that you did have the chemical knowledge?

Gofman: Yes. I think it made lots of difference. It made lots of difference there and I think subsequently, too. I think it's valuable. I'd recommend it except, as you know, getting a Ph.D. is a time-consuming effort; since life is limited maybe one asks is there really a necessity for one person to get a Ph.D. in a discipline like chemistry or biochemistry or any other discipline, and then a medical degree? It just about doubles the time that most people spend in their graduate efforts to prepare for a career.

But then I wonder, if you're willing to do it, why it's so important to be in your career four years early. So I think it's a good thing. Quite a number have done it since the days when there were only a few. M.D./Ph.D.s are maybe not a dime a dozen now, but they're close to that. There are lots of them. We had lots of them who had their M.D. get their Ph.D. at Donner. I thought the chemistry background was just excellent for me. And I think a lot of the M.D.s that came to a multidisciplinary place like Donner got a very good exposure when they worked for a Ph.D. in medical physics or biophysics.

If I had it to do again, I think I would still go the chemistry route, because I'm partial to chemistry. I have no regrets. I think it's good to work in a place where there are various points of view. I don't think the same thing could come out of a department of medicine, where every person had a medical training and, say, a residency and there weren't people with diverse backgrounds. That would be my view.

Hughes: Well, that's about all I have to ask, Dr. Gofman. Do you have anything you care to add?

Gofman: No, I think we've covered just about everything.

[end of interview]

Index - John Gofman

Abell, Liese Lewis, 89
Althausen, Theodore Leon, 67, 68
Alvarez, Luis W., 185
American Cancer Society, 205
American Mercury, 79-80
American Type Culture Collection, 126
Anspaugh, Lynn, 126
Atomic Energy Commission (AEC), 25, 64, 95-98, 104, 109, 117, 119-25, 131-33, 135, 141, 142, 143, 148-53, 156-57, 158, 168, 178-81, 183, 186, 188, 201, 210, 215, 222, 228, 229, 233, 234, 235, 237
Axelrod, Dorothy, 46, 49

Bachmann, Werner, 16-17
Batzel, Roger, 117, 137-38, 143, 178-81
Bauser, Edward, 140
Beierwaltes, William Henry, 193
BEIR Committee, 151, 153-55, 165, 175-76
Belmont Medical Laboratory, 238
Berlin, Nat, 179, 223, 240-42
Biehl, Arthur, 247-48
Biggs, Max W., 97, 116
Birge, Raymond, 61-62, 71, 241
Bjorkland, Russell, 176
Born, James, 106, 237
Boveri, Theodore, 125-26
Bowden, Mark, 153-54
Bray, William, 28
Brewer, Leo, 24, 27-28
Briggs, Leroy, 182-83
Brookhaven National Laboratory, 104
Brown, Edmund G., 231-32
Brown, Harold, 117
Bugher, John, 96, 97, 210, 215
Bush, Vannevar, 37

Calvin, Melvin, 50, 74-75, 221-26
Campbell, Arthur, 17, 22, 24
Cardiodynamics [corporation], 248-49
Case Western Reserve University, 3, 5-6. See also Western Reserve Medical School
Cassperson, , 129
CBS News, 134
Chaikoff, I. Leon, 257-58
Chapman, Robert, 248-50
Cleveland Clinic, 79,81
Cleveland Foundation, 19-20
Committee for Nuclear Responsibility, 163-65, 175
Committee on the Biological Effects of Ionizing Radiation (National Academy of Sciences), 150

Compton, Arthur, 36, 37, 38, 53, 58
Congressional Record, 165
Connick, Robert E., 43
Corbett, Ruth, 21
Coronary Heart Disease (Gofman), 87
Crocker Laboratory (UCB), 44, 49-50

Davis, Loyal, 69
Defense Nuclear Agency (U.S.), 210-11
De Lalla, Oliver, 82, 83, 93
Department of Energy (U.S.), 157, 201, 235
Dobson, Ernest, 57, 187,
Dobson, R. Lowry, 187, 214
Donner, Robert, 218
Donner Foundation, 217-18
Donner Laboratory (UCB), 57-58, 60-62, 72-73, 93, 96, 98-100, 102-104, 109-114, 155-57, 176-82, 185, 187, 188, 190-91, 208-210, 213-14, 216-21, 22, 227-30, 235-37, 240-43
Duffield, Robert, 25, 27, 38, 40-41, 46, 49, 50
Dunham, Charles, 97, 123, 210
Dunning, J.R., 37
Du Pont Chemical Corporation, 54
Durbin, Patricia, 46

Early History of Heavy Isotope Research at Berkeley (Seaborg), 33-34
Edsall, John, 76-77
Edson, Sadie, 78, 85
Ehrlich, Paul, 163
Electrocardiographic Associates, 251-52
Eliott, Harold, 78
Energy Research and Development Agency (U.S.), 152-53, 157, 201
English, Spofford, 25, 27, 29, 32, 38, 40, 50

Fajans, Kasimir, 16
Faulkner, Ernest, 65, 66, 182
Federal Radiation Council (U.S.), 211
Fermi, Enrico, 37
Finch, Robert, 149
Fletcher, Arthur, 9
Ford, Daniel, 148-49
Ford Foundation, 174-75
Forsham, Peter A., 68
Foster, John, 95, 116, 117-24, 136, 146
Fredrickson, Don, 90, 99
Freeman, Keith, 217
Free Speech Movement, 113
Freitas, , 193-95

Garner, Clifford Symes, 40-41
Geesaman, Don, 174-75
Giauque, William F., 26, 31, 33
Gofman, David (father), 1-3, 4-6, 11, 13
Gofman, Helen Fahl (wife), 23, 30, 40-41, 48, 49, 53, 54
Gofman, Sarah (mother), 1-2, 4
Goodrich, Jim, 22, 24
Gravel, Mike, 149, 165
Griggs, Helen, 32

Hamilton, Joe, 44, 46-47, 49, 71, 185, 189, 212
Hanafin, Maurice, 238
Hanford National Laboratory, 53, 54, 104, 185
Harrison, Don, 249
Harvard University, 10-12, 79, 81
Hayes, Thomas, 92, 96, 98, 111, 217
Hayflick, Leonard, 127
Hayworth, Leland, 123
Health Physics, 185
Hewitt, John, 96, 100
Hearst, Catherine, 70
Highland Hospital (Oakland, Cal.), 213-14
Hoerr, Norman, 21-22
Holford, , 160
Holifield, Chester, 140-41, 148
Hollinger, Betty, 252
Holmes, Harry N., 21
Hooper Foundation, 61
Hosmer, Craig, 140
Howard, Frank, 132-33

Institute for Paper Chemistry, 13, 14
Institute of Electrical and Electronics Engineers (IEEE),
  135, 139, 140, 142
Institute of Medical Physics, 237-40
Itano, Harvey A., 225

Jacobs, Paul, 137
Joint Committee on Atomic Energy (U.S. Congress), 140
Jones, Hardin, 57-58, 64, 66, 71, 88, 89, 97, 99, 101-106,
  108, 109, 111, 113, 206, 210, 214, 216, 219-21, 227-31,
  235-37, 239, 255, 257
Jones, Helen, 228-29
Jones, T. Duckett, 81
Journal of Biological Chemistry, 76
Journal of Clinical Endocrinology and Metabolism, 203
Journal of the Canadian Association of Radiologists, 207-208
Journal of the Society of Nuclear Medicine, 189

Kamen, Martin, 50, 58-60
Kathren, Ron, 226-27
Kato, Hiroo, 161-63, 171
Kelly, Lola, 57
Kelly, Orville, 210
Kendall, Forrest Everett, 89
Kendall, Henry Way, 148-49, 151
Kennedy, Joe W., 28-29, 33, 40-41, 42, 45, 58, 60
Kerr, Clark, 122-23
Kerr, William, 61, 65-67, 68, 182
Kittredge, Mabel, 23-24
Kneale, George, 159
Kolb, Felix O., 68, 182
Kovich, Erma, 181

Land, Charles E., 158, 160
Lasker, Mary, 80-81, 98, 232-33
Latimer, Wendell, 28, 40, 42, 43, 54, 225
Lawhorn, Richard, 249, 251, 253
Lawrence, Ernest O., 30, 33, 36-37, 43, 49, 51-53, 68, 69, 72, 97, 106-108, 113, 115, 185-86, 188, 212, 232-33, 234
Lawrence, John L., 44, 49, 51, 57, 58, 60, 62, 64, 65, 68, 70-72, 91, 92, 97-100, 104-112, 122, 185, 189, 190, 201-202, 209, 210, 213-14, 216-19, 222-24, 228-42
Lawrence Berkeley Laboratory, 64, 120
Lawrence Livermore Laboratory, 91, 110, 112, 113-24, 129, 133, 136-37, 141, 143, 144-48, 155, 172, 176, 187
Levy, Bob, 90, 100
Lewis, Gilbert N., 23-25, 225
Libby, William, 234, 235
Lindgren, Frank, 66, 73, 74, 76, 78, 82, 87, 92, 111, 217, 220, 242
Linos, Athena, 167-71
Lipkin, Dave, 58
"Lipoproteins and Atherosclerosis" (Gofman), 65-67
Los Alamos National Laboratory, 40-41, 54, 58
Los Angeles Times, 134
Lyon, Thomas P., 66, 78

Mainhardt, Robert, 247-48
Malamud, Nathan, 68, 182
Manhattan Engineering District, 35, 36
Manhattan Project, 41, 42-48, 54, 55, 187
Marshall, Lenore, 163
Massachusetts Institute of Technology, 148
May, Michael, 136-39, 142-45, 178
Mayo Clinic, 59-60, 167-69, 195
McEwen, Robert, 20-21
McGregor, , 158, 160
McMillan, Edwin, 95
Medical Physics, Division of (UCB), 61-62, 63-71, 104, 190-91, 227-30, 239, 243

Meijer, Robert, 14
Metallurgical Laboratory (Chicago), 37, 39, 53
Metzenbaum, Howard, 7, 10
Miller, Earl, 185-87
Minkler, Jay, 128
Minogue, Robert, 166
Miter Corporation, 144-45
Moore, Felix, 88, 89
Morris, Jerry, 91-92
Mortimer, Robert K., 185
Mumford, Lewis, 163
Muskie, Edmund, 139-40, 142, 149

Nader, Ralph, 149
Naffziger, Howard C., 56-57, 64, 69
Napa State Hospital (Cal.), 94
National Academy of Sciences, 149-50, 153
National Cancer Institute (NCI), 128, 158, 179-81, 241
National Heart Institute, 79-81, 86, 88, 90, 100
National Institutes of Health (NIH), 79, 87, 90, 98, 99
National Youth Administration, 9, 12
Natural Resources Defense Council, 175
Nature of the Chemical Bond, The (Pauling), 225
Neurath, Hans, 245
Nevada State Board of Health, 101
New England Journal of Medicine, 167-69
Newhouse News Service, 132-33
New York Academy of Medicine, 232
New York Times, 10-11
Neylan, John Francis, 69-71
Nichols, Alexander V., 66, 82, 83, 86, 87, 90, 92, 98, 100,
  111, 121, 185, 217, 220, 221, 242
Nolan, Bill, 197
Nuclear Regulatory Commission, 152, 166
Nucleonics Week, 173-74

Oak Ridge National Laboratory, 53-54, 104
Oberlin College, 7-10, 12-26, 20-21
Office of Scientific Research and Development (U.S.), 31
Olson, Axel Ragnar, 26, 30
Oppenheimer, J. Robert, 25, 27-28, 40-41, 42-43, 45, 53
Osgood, Edwin, 59
"Oxford Study" (Stewart), 159-63

Pacific Union Club, 69
Page, Irvine, 81
Pauling, Linus, 118, 119, 225-26, 234, 254, 258
Pederson, Kai, 74, 76-77, 253
Phillips, Ken, 174
Pickels, Ed Greydon, 74, 238
Pitzer, Kenneth, 24, 26, 225
Ploughshare, 139, 235
Poisoned Power (Gofman), 139
Polster, Harry Deleon, 14

Price, Melvin, 140

Radford, Edward P., 153-54
Radiation and Human Health (Gofman), 157, 166-67, 198, 206-207
Rand Corporation, 172
Randall, Merle, 43
Randolph Field (USAF), 214-15
Rauscher, Frank, 179-80
Rensselaer Polytechnic Institute, 4-6
Reynolds, Wallace, 233, 235
Rollefson, Ragnar, 26
Rosenburg scholarship, 41
Rosenthal, Don J., 97
Rossi, Harald H., 154
Rubin, Sam, 50

Sanford, Katherine Koontz, 128
San Souci, Bob, 236-37
Science, 65, 225
Scott, Kenneth C., 209
Scott, Wendell, 59
Seaborg, Glenn T., 26, 28-41, 49, 58, 89, 103, 117, 123-25, 131, 133-34, 222, 234
Seaman, William, 8
Shaefer, Elizabeth, 175
Shaw, Milton, 149
Sherman, Ralph, 238-39
Shore, Bernard, 146, 176-77
Shore Virgie, 176
Simon, Alex, 68, 182
Simonton, John, 97
Smith, Lloyd H. (Holly), 183
Smyth, Francis, 55
Soderberg, Margaret, 181
Sollman, Torald, 18-19, 23
Sonoma State Hospital (Cal.), 94
Spector, David, 239
Spedding, Frank, 37, 38
Spinco, 74, 238
Spivak, Larry, 79-80
Steiner, Luke Eby, 12-15, 21, 22, 31
Stevenson, Adlai, 232-33, 235
Stewart, Alice, 159-63, 234
Stone, Robert, 55, 63-64, 157, 185, 187, 190
Stone, Stuart, 129
Stouffer Prize, 135
Stoughton, Ray W., 31
Strisower, Beverly, 82, 88

Tamplin, Arthur, 82, 100, 134, 135, 136, 139-45, 147, 149, 150, 172-76

Taylor, Kenneth, 207-208
Teller, Edward, 119
Thermodynamics (Lewis and Randall), 24, 43
Three Mile Island, 152, 165-66
Tobias, Cornelius, 71, 97, 99, 100, 109-111, 185, 206, 228
Todd, T. Wingate, 22
Totter, John Randolph, 132-36, 210
Trautman, Rodes, 222-23, 225-26

Ultracentrifugation of Serum (Pederson), 74
Underhill, Robert, 123, 229-30
Union of Concerned Scientists, 149
U.S. Air Force, 214-16, 234
U.S. Navy, 55-56
U.S. Public Health Service, 135-36
University of California (Berkeley), 17, 22, 23-28, 58, 60, 62, 243-44
University of California (Berkeley) Academic Senate, 241
University of California (Berkeley) School of Law, 174-75
University of California Board of Regents, 69-71, 120-23
University of California (San Francisco) Medical School, 53-58, 60-71, 182-83, 243-44, 245
University of Miami, 245
University of Michigan, 16-17
University of Oregon Medical School, 59-60
University of Washington (Seattle), 245-46
Urey, Harold, 37

Van Dyke, Donald, 223

Wahl, Arthur C., 29, 32, 34, 36, 37-41, 42, 46, 47, 58, 59
Warren, Shields, 96-97, 210
Warren, Stafford, 45
Washington University, 58-60
Weissman, Sam, 58
Western Reserve Medical School, 17-22, 23, 54-55
What We Do Know About Heart Attacks (Gofman), 90
Wilmarth, Wayne, 50

York, Herbert, 115

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